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THE SCIENTIFIC MONTHLY

OCTOBER, 1920

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE[1]

OCEANOGRAPHY AND THE SEA-FISHERIES

By Professor WILLIAM A. HERDMAN

PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

OCEANOGRAPHY has many practical applications, chiefly, but by no means wholly, on the biological side. The great fishing industries of the world deal with living organisms, of which all the vital activities and the inter-relations with the environment are matters of scientific investigation. Aquiculture is as susceptible of scientific treatment as agriculture can be; and the fisherman, who has been in the past too much the nomad and the hunter—if not, indeed, the devastating raider—must become in the future the settled farmer of the sea if his harvest is to be less precarious. Perhaps the nearest approach to cultivation of a marine product, and of the fisherman reaping what he has actually sown, is seen in the case of the oyster and mussel industries on the west coast of France, in Holland, America, and, to a less extent, on our own coast. Much has been done by scientific men for these and other similar coastal fisheries since the days when Prof. Coste in France in 1859 introduced oysters from the Scottish oyster-beds to start the great industry at Areachon and elsewhere. Now we buy back the descendants of our own oysters from the French ostreiculturists to replenish our depleted beds.

It is no small matter to have introduced a new and important food-fish to the markets of the world. The remarkable deep-water "tile-fish," new to science and described as *Lopholatilus chamæleonticeps*, was discovered in 1879 by one of the United States fishing schooners to the south of Nantucket, near the 100-fathom line. Several thousand pounds weight were

[1] Extracts from addresses given at the Cardiff Meeting.

caught, and the matter was duly investigated by the United States Fish Commission. For a couple of years after that the fish was brought to market in quantity, and then something unusual happened at the bottom of the sea, and in 1882 millions of dead tile-fish were found floating on the surface over an area of thousands of square miles. The schooner *Navarino* sailed for two days and a night through at least 150 miles of sea, thickly covered as far as the eye could reach with dead fish, estimated at 256,000 to the square mile. The Fish Commission sent a vessel to fish systematically over the grounds known as the "Gulf Stream slope," where the tile-fish had been so abundant during the two previous years, but she did not catch a single fish, and the associated sub-tropical invertebrate fauna was also practically obliterated.

This wholesale destruction was attributed by the American oceanographers to a sudden change in the temperature of the water at the bottom, due in all probability to a withdrawal southwards of the warm Gulf Stream water and a flooding of the area by the cold Labrador current.

I am indebted to Dr. C. H. Townsend, Director of the celebrated New York Aquarium, for the latest information in regard to the reappearance in quantity of this valuable fish upon the old fishing grounds off Nantucket and Long Island, at about 100 miles from the coast to the east and southeast of New York. It is believed that the tile-fish is now abundant enough to maintain an important fishery, which will add an excellent food-fish to the markets of the United States. It is easily caught with lines at all seasons of the year, and reaches a length of over three feet and a weight of 40 to 50 pounds. During July, 1915, the product of the fishery was about two and a half million pounds weight, valued at 55,000 dollars, and in the first few months of 1917 the catch was four and a half million pounds, for which the fishermen received 247,000 dollars.

We can scarcely hope in European seas to add new food-fishes to our markets, but much may be done through the cooperation of scientific investigators of the ocean with the Administrative Departments to bring about a more rational conservation and exploitation of the national fisheries.

Earlier in this address I referred to the pioneer work of the distinguished Manx naturalist, Professor Edward Forbes. There are many of his writings and of his lectures which I have no space to refer to which have points of oceanographic interest. Take this, for example, in reference to our national sea fisheries. We find him in 1847 writing to a friend:

On Friday night I lectured at the Royal Institution. The subject was the bearing of submarine researches and distribution matters on the fishery question. I pitched into government mismanagement pretty strong, and made a fair case of it. It seems to me that at a time when half the country is starving we are utterly neglecting or grossly mismanaging great sources of wealth and food. . . . Were I a rich man I would make the subject a hobby, for the good of the country and for the better proving that the true interests of government are those linked with and inseparable from science.

We must still cordially approve of these last words, while recognizing that our Government Department of Fisheries is now being organized on better lines, is itself carrying on scientific work of national importance, and is, I am happy to think, in complete sympathy with the work of independent scientific investigators of the sea and desirous of closer cooperation with university laboratories and biological stations.

During recent years one of the most important and most frequently discussed of applications of fisheries investigation has been the productivity of the trawling grounds, and especially those of the North Sea. It has been generally agreed that the enormous increase of fishing power during the last forty years or so has reduced the number of large plaice, so that the average size of that fish caught in our home waters has become smaller, although the total number of plaice landed had continued to increase up to the year of the outbreak of war. Since then, from 1914 to 1919, there has of necessity been what may be described as the most gigantic experiment ever seen in the closing of extensive fishing grounds. It is still too early to say with any certainty exactly what the results of that experiment have been, although some indications of an increase of the fish population in certain areas have been recorded. For example, the Danes, A. C. Johansen and Kirstine Smith, find that large plaice landed in Denmark are now more abundant, and they attribute this to a reversal of the pre-war tendency, due to less intensive fishing. But Dr. James Johnstone has pointed out that there is some evidence of a natural periodicity in abundance of such fish and that the results noticed may represent phases in a cyclic change. If the periodicity noted in Liverpool Bay[2] holds good for other grounds it will be necessary in any comparison of pre-war and post-war statistics to take this natural variation in abundance into very careful consideration.

In the application of oceanographic investigations to sea-fisheries problems, one ultimate aim, whether frankly admitted

[2] See Johnstone, *Report Lancs. Sea-Fish. Lab.* for 1917, p. 60; and Daniel, *Report* for 1919, p. 51.

or not, must be to obtain some kind of a rough approximation to a census or valuation of the sea—of the fishes that form the food of man, of the lower animals of the sea-bottom on which many of the fishes feed, and of the planktonic contents of the upper waters which form the ultimate organized food of the sea—and many attempts have been made in different ways to attain the desired end.

Our knowledge of the number of animals living in different regions of the sea is for the most part relative only. We know that one haul of the dredge is larger than another, or that one locality seems richer than another, but we have very little information as to the actual numbers of any kind of animal per square foot or per acre in the sea. Hensen, as we have seen, attempted to estimate the number of food-fishes in the North Sea from the number of their eggs caught in a comparatively small series of hauls of the tow-net, but the data were probably quite insufficient and the conclusions may be erroneous. It is an interesting speculation to which we can not attach any economic importance. Heincke says of it:

> This method appears theoretically feasible, but presents in practise so many serious difficulties that no positive results of real value have as yet been obtained.

All biologists must agree that to determine even approximately the number of individuals of any particular species living in a known area is a contribution to knowledge which may be of great economic value in the case of the edible fishes, but it may be doubted whether Hensen's methods, even with greatly increased data, will ever give us the required information. Petersen's method, of setting free marked plaice and then assuming that the proportion of these recaught is to the total number marked as the fishermen's catch in the same district is to the total population, will only hold good in circumscribed areas where there is practically no migration and where the fish are fairly evenly distributed. This method gives us what has been called "the fishing coefficient," and this has been estimated for the North Sea to have a probable value of about 0.33 for those sizes of fish which are caught by the trawl. Heincke,[3] from an actual examination of samples of the stock on the ground obtained by experimental trawling ("the catch coefficient"), supplemented by the market returns of the various countries, estimates the adult plaice at about 1,500 millions,

[3] F. Heincke, *Cons. Per. Internat. Explor. de la Mer*, "Investigations on the Plaice," Copenhagen, 1913.

of which about 500 millions are caught or destroyed by the fishermen annually.

It is difficult to imagine any further method which will enable us to estimate any such case as, say, the number of plaice in the North Sea where the individuals are so far beyond our direct observation and are liable to change their positions at any moment. But a beginning can be made on more accessible ground with more sedentary animals, and Dr. C. G. Joh. Petersen, of the Danish Biological Station, has for some years been pursuing the subject in a series of interesting reports on the "Evaluation of the Sea."[4] He uses a bottom-sampler, or grab, which can be lowered down open and then closed on the bottom so as to bring up a sample square foot or square meter (or in deep water one tenth of a square meter) of the sand or mud and its inhabitants. With this apparatus, modified in size and weight for different depths and bottoms, Peterson and his fellow-workers have made a very thorough examination of the Danish waters, and especially of the Kattegat and the Limfjord, have described a series of "animal communities" characteristic of different zones and regions of shallow water, and have arrived at certain numerical results as to the quantity of animals in the Kattegat expressed in tons—such as 5,000 tons of plaice requiring as food 50,000 tons of "useful animals" (mollusca and polychæt worms), and 25,000 tons of starfish using up 200,000 tons of useful animals which might otherwise serve as food for fishes, and the dependence of all these animals directly or indirectly upon the great Beds of Zostera, which make up 24,000,000 tons in Kattegat. Such estimates are obviously of great biological interest, and even if only rough approximations are a valuable contribution to our understanding of the metabolism of the sea and of the possibility of increasing the yield of local fisheries.

But on studying these Danish results in the light of what we know of our own marine fauna, although none of our seas has been examined in the same detail by the bottom-sampler method, it seems probable that the animal communities as defined by Petersen are not exactly applicable on our coasts and that the estimates of relative and absolute abundance may be very different in different seas under different conditions. The work will have to be done in each great area, such as the North Sea, the English Channel, and the Irish Sea, independently. This is a necessary investigation, both biological and physical,

[4] See *Reports of the Danish Biological Station*, and especially the *Report* for 1918 "The Sea Bottom and its Production of Fish Food."

which lies before the oceanographers of the future, upon the results of which the future preservation and further cultivation of our national sea-fisheries may depend.

It has been shown by Johnstone and others that the common edible animals of the shore may exist in such abundance that an area of the sea may be more productive of food for man than a similar area of pasture or crops on land. A Lancashire mussel bed has been shown to have as many as 16,000 young mussels per square foot, and it is estimated that in the shallow waters of Liverpool Bay there are from twenty to 200 animals of sizes varying from an amphipod to a plaice on each square meter of the bottom.[5]

From these and similar data which can be readily obtained, it is not difficult to calculate totals by estimating the number of square yards in areas of similar character between tide-marks or in shallow water. And from weighings of samples some approximation to the number of tons of available food may be computed. But one must not go too far. Let all the figures be based upon actual observation. Imagination is necessary in science, but in calculating a population of even a very limited area it is best to believe only what one can see and measure.

Countings and weighings. however, do not give us all the information we need. It is something to know even approximately the number of millions of animals on a mile of shore and the number of millions of tons of possible food in a sea-area, but that is not sufficient. All food-fishes are not equally nourishing to man, and all plankton and bottom invertebrata are not equally nourishing to a fish. At this point the biologist requires the assistance of the physiologist and the bio-chemist. We want to know next the value of our food matters in proteids, carbohydrates, and fats, and the resulting calories. Dr. Johnstone, of the oceanography department of the University of Liverpool, has already shown us how markedly a fat summer herring differs in essential constitution from the ordinary white fish, such as the cod, which is almost destitute of fat.

Professor Brandt, at Kiel, Professor Benjamin Moore, at Port Erin, and others have similarly shown that plankton gatherings may vary greatly in their nutrient value according as they are composed mainly of Diatoms, of Dinoflagellates, or of Copepoda. And, no doubt, the animals of the "benthos," the common invertebrates of our shores, will show similar differ-

[5] "Conditions of Life in the Sea," Cambridge Univ. Press, 1908.

ences in analysis.[6] It is obvious that some contain more solid flesh, others more water in their tissues, others more calcareous matter in the exoskeleton, and that therefore weight for weight we may be sure that some are more nutritious than the others; and this is probably at least one cause of that preference we see in some of our bottom-feeding fish for certain kinds of food, such as polychæt worms, in which there is relatively little waste, and thin-shelled lamellibranch molluscs, such as young mussels, which have a highly nutrient body in a comparatively thin and brittle shell.

My object in referring to these still incomplete investigations is to direct attention to what seems a natural and useful extension of faunistic work, for the purpose of obtaining some approximation to a quantitative estimate of the more important animals of our shores and shallow water and their relative values as either the immediate or the ultimate food of marketable fishes.

Each such fish has its "food-chain" or series of alternative chains, leading back from the food of man to the invertebrates upon which it preys and then to the food of these, and so down to the smallest and simplest organisms in the sea, and each such chain must have all its links fully worked out as to seasonal and quantitative occurrence back to the Diatoms and Flagellates which depend upon physical conditions and take us beyond the range of biology—but not beyond that of oceanography. The Diatoms and the Flagellates are probably more important than the more obvious sea-weeds not only as food, but also in supplying to the water the oxygen necessary for the respiration of living protoplasm. Our object must be to estimate the rate of production and rate of destruction of all organic substances in the sea.

To attain to an approximate census and valuation of the sea—remote though it may seem—is a great aim, but it is not sufficient. We want not only to observe and to count natural objects, but also to understand them. We require to know not merely what an organism is—in the fullest detail of structure and development and affinities—and also where it occurs—again in full detail—and in what abundance under different circumstances, but also *how* it lives and what all its relations are to

[6] Moore and others have made analyses of the protein, fat, etc., in the soft parts of Sponge, Ascidian, Aplysia, Fusus, Echinus and Cancer at Port Erin, and find considerable differences—the protein ranging, for example, from 8 to 51 per cent., and the fat from 2 to 14 per cent. (see *Bio-Chemical Journ.*, VI., p. 291).

both its physical and its biological environment, and that is where the physiologist, and especially the bio-chemist, can help us. In the best interests of biological progress the day of the naturalist who merely collects, the day of the anatomist and histologist who merely describe, is over, and the future is with the observer and the experimenter animated by a divine curiosity to enter into the life of the organism and understand how it lives and moves and has its being. "Happy indeed is he who has been able to discover the causes of things."

Cardiff is a sea-port, and a great sea-port, and the Bristol Channel is a notable sea-fisheries center of growing importance. The explorers and merchant venturers of the southwest of England are celebrated in history. What are you doing now in Cardiff to advance our knowledge of the ocean? You have here an important university center and a great modern national museum, and either or both of these homes of research might do well to establish an oceanographical department, which would be an added glory to your city and of practical utility to the country. This is the obvious center in Wales for a sea-fisheries institute for both research and education. Many important local movements have arisen from British Association meetings, and if such a notable scientific development were to result from the Cardiff meeting of 1920, all who value the advance of knowledge and the application of knowledge to industry would applaud your enlightened action.

But in a wider sense, it is not to the people of Cardiff alone that I appeal, but to the whole population of these Islands, a maritime people who owe everything to the sea. I urge them to become better informed in regard to our national sea-fisheries and take a more enlightened interest in the basal principles that underlie a rational regulation and exploitation of these important industries. National efficiency depends to a very great extent upon the degree in which scientific results and methods are appreciated by the people and scientific investigation is promoted by the government and other administrative authorities. The principles and discoveries of science apply to aquiculture no less than to agriculture. To increase the harvest of the sea the fisheries must be continuously investigated, and such cultivation as is possible must be applied, and all this is clearly a natural application of the biological and hydrographical work now united under the science of oceanography.

THE INTERNAL CONSTITUTION OF THE STARS

By Professor A. S. EDDINGTON

PRESIDENT OF THE MATHEMATICAL AND PHYSICAL SCIENCE SECTION

THERE is another line of astronomical evidence which appears to show more definitely that the evolution of the stars proceeds far more slowly than the contraction hypothesis allows; and perhaps it may ultimately enable us to measure the true rate of progress. There are certain stars, known as Cepheid variables, which undergo a regular fluctuation of light of a characteristic kind, generally with a period of a few days. This light change is *not* due to eclipse. Moreover, the color quality of the light changes between maximum and minimum, evidently pointing to a periodic change in the physical condition of the star. Although these objects were formerly thought to be double stars, it now seems clear that this was a misinterpretation of the spectroscopic evidence. There is in fact no room for the hypothetical companion star; the orbit is so small that we should have to place it inside the principal star. Everything points to the period of the light pulsation being something intrinsic in the star; and the hypothesis advocated by Shapley, that it represents a mechanical pulsation of the star, seems to be the most plausible. I have already mentioned that the observed period does in fact agree with the calculated period of mechanical pulsation, so that the pulsation explanation survives one fairly stringent test. But whatever the cause of the variability, whether pulsation or rotation, provided only that it is intrinsic in the star, and not forced from outside, the density must be the leading factor in determining the period. If the star is contracting so that its density changes appreciably, the period can not remain constant. Now, on the contraction hypothesis the change of density must amount to at least 1 per cent. in 40 years. (I give the figures for δ Cephei, the best-known variable of this class.) The corresponding change of period should be very easily detectable. For δ Cephei the period ought to decrease 40 seconds annually.

Now δ Cephei has been under careful observation since 1785, and it is known that the change of period, if any, must be very small. S. Chandler found a decrease of period of $\frac{1}{20}$ second per annum, and in a recent investigation E. Hertzsprung has found a decrease of $\frac{1}{10}$ second per annum. The evidence that there is any decrease at all rests almost entirely on the earliest obser-

vations made before 1800, so that it is not very certain; but in any case the evolution is proceeding at not more than $\frac{1}{400}$ of the rate required by the contraction hypothesis. There must at this stage of the evolution of the star be some other source of energy which prolongs the life of the star 400-fold. The time-scale so enlarged would suffice for practically all reasonable demands.

I hope the dilemma is plain. Either we must admit that whilst the density changes 1 per cent. a certain period intrinsic in the star can change no more than $\frac{1}{800}$ of 1 per cent., or we must give up the contraction hypothesis.

If the contraction theory were proposed today as a novel hypothesis I do not think it would stand the smallest chance of acceptance. From all sides—biology, geology, physics, astronomy—it would be objected that the suggested source of energy was hopelessly inadequate to provide the heat spent during the necessary time of evolution; and, so far as it is possible to interpret observational evidence confidently, the theory would be held to be definitely negatived. Only the inertia of tradition keeps the contraction hypothesis alive—or rather, not alive, but an unburied corpse. But if we decided to inter the corpse, let us frankly recognize the position in which we are left. A star is drawing on some vast reservoir of energy by means unknown to us. This reservoir can scarcely be other than the sub-atomic energy which, it is known, exists abundantly in all matter; we sometimes dream that man will one day learn how to release it and use it for his service. The store is well-nigh inexhaustible, if only it could be tapped. There is sufficient in the sun to maintain its output of heat for 15 billion years.

Certain physical investigations in the past year, which I hope we may hear about at this meeting, make it probable to my mind that some portion of this sub-atomic energy is actually being set free in the stars. F. W. Aston's experiments seem to leave no room for doubt that all the elements are constituted out of hydrogen atoms bound together with negative electrons. The nucleus of the helium atom, for example, consists of 4 hydrogen atoms bound with 2 electrons. But Aston has further shown conclusively that the mass of the helium atom is less than the sum of the masses of the 4 hydrogen atoms which enter into it; and in this at any rate the chemists agree with him. There is a loss of mass in the synthesis amounting to about 1 part in 120, the atomic weight of hydrogen being 1.008 and that of helium just 4. I will not dwell on his beautiful proof of this, as you will no doubt be able to hear it from himself. Now

mass can not be annihilated, and the deficit can only represent the mass of the electrical energy set free in the transmutation. We can therefore at once calculate the quantity of energy liberated when helium is made out of hydrogen. If 5 per cent. of a star's mass consists initially of hydrogen atoms, which are gradually being combined to form more complex elements, the total heat liberated will more than suffice for our demands, and we need look no further for the source of a star's energy.

But is it possible to admit that such a transmutation is occurring? It is difficult to assert, but perhaps more difficult to deny, that this is going on. Sir Ernest Rutherford has recently been breaking down the atoms of oxygen and nitrogen, driving out an isotope of helium from them; and what is possible in the Cavendish laboratory may not be too difficult in the sun. I think that the suspicion has been generally entertained that the stars are the crucibles in which the lighter atoms which abound in the nebulæ are compounded into more complex elements. In the stars matter has its preliminary brewing to prepare the greater variety of elements which are needed for a world of life. The radio-active elements must have been formed at no very distant date; and their synthesis, unlike the generation of helium from hydrogen, is endothermic. If combinations requiring the addition of energy can occur in the stars, combinations which liberate energy ought not to be impossible.

We need not bind ourselves to the formation of helium from hydrogen as the sole reaction which supplies the energy, although it would seem that the further stages in building up the elements involve much less liberation, and sometimes even absorption, of energy. It is a question of accurate measurement of the deviations of atomic weights from integers, and up to the present hydrogen is the only element for which Mr. Aston has been able to detect the deviation. No doubt we shall learn more about the possibilities in due time. The position may be summarized in these terms: the atoms of all elements are built of hydrogen atoms bound together, and presumably have at one time been formed from hydrogen; the interior of a star seems as likely a place as any for the evolution to have occurred; whenever it did occur a great amount of energy must have been set free; in a star a vast quantity of energy is being set free which is hitherto unaccounted for. You may draw a conclusion if you like.

If, indeed, the sub-atomic energy in the stars is being freely used to maintain their great furnaces, it seems to bring a little nearer to fulfilment our dream of controlling this latent power for the well-being of the human race—or for its suicide.

So far as the immediate needs of astronomy are concerned, it is not of any great consequence whether in this suggestion we have actually laid a finger on the true source of the heat. It is sufficient if the discussion opens our eyes to the wider possibilities. We can get rid of the obsession that there is no other conceivable supply besides contraction, but we need not again cramp ourselves by adopting prematurely what is perhaps a still wilder guess. Rather we should admit that the source is not certainly known, and seek for any possible astronomical evidence which may help to define its necessary character. One piece of evidence of this kind may be worth mentioning. It seems clear that it must be the high temperature inside the stars which determines the liberation of energy, as H. N. Russell has pointed out. If so the supply may come mainly from the hottest region at the center. I have already stated that the general uniformity of the opacity of the stars is much more easily intelligible if it depends on scattering rather than on true absorption; but it did not seem possible to reconcile the deduced stellar opacity with the theoretical scattering coefficient. Within reasonable limits it makes no great difference in our calculations at what parts of the star the heat energy is supplied, and it was assumed that it comes more or less evenly from all parts, as would be the case on the contraction theory. The possibility was scarcely contemplated that the energy is supplied entirely in a restricted region round the center. Now, the more concentrated the supply, the lower is the opacity requisite to account for the observed radiation. I have not made any detailed calculations, but it seems possible that for a sufficiently concentrated source the deduced and the theoretical coefficients could be made to agree, and there does not seem to be any other way of accomplishing this. Conversely, we might perhaps argue that the present discrepancy of the coefficients shows that the energy supply is not spread out in the way required by the contraction hypothesis, but belongs to some new source only available at the hottest, central part of the star.

I should not be surprised if it is whispered that this address has at times verged on being a little bit speculative; perhaps some outspoken friend may bluntly say that it has been highly speculative from beginning to end. I wonder what is the touchstone by which we may test the legitimate development of scientific theory and reject the idly speculative. We all know of theories which the scientific mind instinctively rejects as fruitless guesses; but it is difficult to specify their exact defect or to supply a rule which will show us when we ourselves do err.

It is often supposed that to speculate and to make hypotheses are the same thing; but more often they are opposed. It is when we let our thoughts stray outside venerable, but sometimes insecure, hypotheses that we are said to speculate. Hypothesis limits speculation. Moreover, distrust of speculation often serves as a cover for loose thinking; wild ideas take anchorage in our minds and influence our outlook; whilst it is considered too speculative to subject them to the scientific scrutiny which would exorcise them.

If we are not content with the dull accumulation of experimental facts, if we make any deductions or generalizations, if we seek for any theory to guide us, some degree of speculation can not be avoided. Some will prefer to take the interpretation which seems to be most immediately indicated and at once adopt that as a hypothesis; others will rather seek to explore and classify the widest possibilities which are not definitely inconsistent with the facts. Either choice has its dangers; the first may be too narrow a view and lead progress into a cul-de-sac; the second may be so broad that it is useless as a guide, and diverges indefinitely from experimental knowledge. When this last case happens, it must be concluded that the knowledge is not yet ripe for theoretical treatment and speculation is premature. The time when speculative theory and observational research may profitably go hand in hand is when the possibilities, or at any rate the probabilities, can be narrowed down by experiment, and the theory can indicate the tests by which the remaining wrong paths may be blocked up one by one.

The mathematical physicist is in a position of peculiar difficulty. He may work out the behavior of an ideal model of material with specifically defined properties, obeying mathematically exact laws, and so far his work is unimpeachable. It is no more speculative than the binomial theorem. But when he claims a serious interest for his toy, when he suggests that his model is like something going on in Nature, he inevitably begins to speculate. Is the actual body really like the ideal model? May not other unknown conditions intervene? He can not be sure, but he can not suppress the comparison; for it is by looking continually to Nature that he is guided in his choice of a subject. A common fault, to which he must often plead guilty, is to use for the comparison data over which the more experienced observer shakes his head; they are too insecure to build extensively upon. Yet even in this, theory may help observation by showing the kind of data which it is especially important to improve.

I think that the more idle kinds of speculation will be avoided if the investigation is conducted from the right point of view. When the properties of an ideal model have been worked out by rigorous mathematics, all the underlying assumptions being clearly understood, then it becomes possible to say that such and such properties and laws lead precisely to such and such effects. If any other disregarded factors are present, they should now betray themselves when a comparison is made with Nature. There is no need for disappointment at the failure of the model to give perfect agreement with observation; it has served its purpose, for it has distinguished what are the features of the actual phenomena which require new conditions for their explanation. A general preliminary agreement with observation is necessary, otherwise the model is hopeless; not that it is necessarily wrong so far as it goes, but it has evidently put the less essential properties foremost. We have been pulling at the wrong end of the tangle, which has to be unravelled by a different approach. But after a general agreement with observation is established, and the tangle begins to loosen, we should always make ready for the next knot. I suppose that the applied mathematician whose theory has just passed one still more stringent test by observation ought not to feel satisfaction, but rather disappointment—"Foiled again! This time I *had* hoped to find a discordance which would throw light on the points where my model could be improved." Perhaps that is a counsel of perfection; I own that I have never felt very keenly a disappointment of this kind.

Our model of Nature should not be like a building—a handsome structure for the populace to admire, until in the course of time some one takes away a corner stone and the edifice comes toppling down. It should be like an engine with movable parts. We need not fix the position of any one lever; that is to be adjusted from time to time as the latest observations indicate. The aim of the theorist is to know the train of wheels which the lever sets in motion—that binding of the parts which is the soul of the engine.

In ancient days two aviators procured to themselves wings. Dædalus flew safely through the middle air across the sea, and was duly honored on his landing. Young Icarus soared upwards towards the sun till the wax melted which bound his wings, and his flight ended in fiasco. In weighing their achievements perhaps there is something to be said for Icarus. The classic authorities tell us that he was only "doing a stunt," but I prefer to think of him as the man who certainly brought to

light a constructional defect in the flying machines of his day. So too in science. Cautious Dædalus will apply his theories where he feels most confident they will safely go; but by his excess of caution their hidden weaknesses can not be brought to light. Icarus will strain his theories to the breaking-point till the weak joints gape. For a spectacular stunt? Perhaps partly; he is often very human. But if he is not yet destined to reach the sun and solve for all time the riddle of its constitution, yet he may hope to learn from his journey some hints to build a better machine.

THE SCIENTIFIC STUDY OF ALLOYS

By C. T. HEYCOCK

PRESIDENT OF THE CHEMICAL SECTION

IN 1897 Neville and I determined the complete freezing-point curve of the copper-tin alloys, confirming and extending the work of Roberts-Austen, Stansfield, and Le Chatelier; but the real meaning of the curve remained as much of a mystery as ever. Early in 1900 Sir G. Stokes suggested to us that we should make a microscopic examination of a few bronzes as an aid to the interpretation of the singularities of the freezing-point curve. An account of this work, which occupied us for more than two years, was published as the Bakerian Lecture of the Royal Society in February, 1903. Whilst preparing a number of copper-tin alloys of known composition we were struck by the fact that the crystalline pattern which developed on the free surface of the slowly cooled alloys was entirely unlike the structure developed by polishing and etching sections cut from the interior; it therefore appeared probable that changes were going on within the alloys as they cooled. In the hope that, as Sorby had shown in the case of steel, we could stereotype or fix the change by sudden cooling, we melted small ingots of the copper in alloys and slowly cooled them to selected temperatures and then suddenly chilled them in water. The results of this treatment were communicated to the Royal Sosiety and published in the *Proceedings*, February, 1901.

To apply this method to a selected alloy we first determined its cooling curve by means of an automatic recorder, the curve usually showing several halts or steps in it. The temperature of the highest of these steps correponded with a point on the liquidus, *i. e.*, when solid first separated out from the molten

mass. To ascertain what occurred at the subsequent halts, ingots of the melted alloy were slowly cooled to within a few degrees above and below the halt and then chilled, with the result just seen on the screen.

The method of chilling also enabled us to fix, with some degree of accuracy, the position of points on the solidus. If an alloy, chilled when it is partly solid and partly liquid, is polished and etched, it will be seen to consist of large primary combs embedded in a matrix consisting of mother liquor, in which are disseminated numerous small combs, which we called "chilled primary." By repeating the process at successively lower and lower temperatures we obtained a point at which the chilled primary no longer formed, *i. e.*, the upper limit of the solidus.

Although we made but few determinations of the physical properties of the alloys, it is needless to say how much they vary with the temperature and with the rapidity with which they are heated or cooled.

From a consideration of the singularities in the liquidus curve, coupled with the microscopic examination of slowly cooled and chilled alloys, we were able to divide the copper-tin alloys into certain groups having special qualities. It would take far too long to discuss these divisions. In interpreting our result we were greatly assisted not only by the application of the phase rule, but also by the application of Roozeboom's theory of solid solution (unfortunately Professor Roozeboom's letters were destroyed by fire in June, 1910) and by the advice he kindly gave us. At the time the paper was published we expressly stated that we did not regard all our results as final, as much more work was required to clear up points still obscure. Other workers—Shepherd and Blough, Giolitti and Tavanti—have somewhat modified the diagram.

Neither Shepherd and Blough nor Hoyt have published the photomicrographs upon which their results are based, so that it is impossible to criticize their conclusions. Giolitti and Tavanti have published some microphotographs, from which it seems that they had not allowed sufficient time for equilibrium to be established. In this connection I must call attention to the excellent work of Haughton on the constitution of the alloys of copper and tin.[1] He investigated the alloys rich in tin, and illustrated his conclusions by singularly beautiful microphotographs, and has done much to clear up doubtful points in this region of the diagram. I have dwelt at some length on this work, for copper-tin is probably the first of the binary alloys on

[1] *Journ. Institute of Metals*, March, 1915.

which an attempt had been made to determine the changes which take place in passing from one pure constituent to the other. I would again call attention to the fact that without a working theory of solution the interpretation of the results would have been impossible.

Since 1900, many complete equilibrium diagrams have been published; amongst them may be mentioned the work of Rosenhain and Tucker on the lead-tin alloys,[2] in which they describe hitherto unsuspected changes on the lead rich side which go on when these alloys are at quite low temperatures, also the constitution of the alloys of aluminum and zinc; the work of Rosenhain and Archbutt,[3] and quite recently the excellent work of Vivian, on the alloys of tin and phosphorus, which has thrown an entirely new light on this difficult subject.

So far I have called attention to some of the difficulties encountered in the examination of binary alloys. When we come to ternary alloys the difficulties of carrying out an investigation are enormously increased, whilst with quaternary alloys they seem almost insurmountable; in the case of steels containing always six, and usually more, constituents, we can only hope to get information by purely empirical methods.

Large numbers of the elements and their compounds which originally were laboriously prepared and investigated in the laboratory and remained dormant as chemical curiosities for many years have, in the fulness of time, taken their places as important and, indeed, essential articles of commerce. Passing over the difficulties encountered by Davy in the preparation of metallic sodium and by Faraday in the production of benzene (both of which materials are manufactured in enormous quantities at the present time), I may remark that even during my own lifetime I have seen a vast number of substances transferred from the category of rare laboratory products to that which comprises materials of the utmost importance to the modern metallurgical industries. A few decades ago, aluminium, chromium, cerium, thorium, tungsten, manganese, magnesium, molybdenum, nickel, calcium and calcium carbide, carborundum, and acetylene, were unknown outside the chemical laboratory of the purely scientific investigator; today these elements, their compounds and alloys, are amongst the most valuable of our industrial metallic products. They are essential in the manufacture of high-speed steels, of armor plate, of filaments for the electric bulb lamp, of incandescent gas

[2] *Phil. Trans.*, 1908.
[3] *Phil. Trans.*, 1911.

mantles, and of countless other products of modern scientific industry.

All these metallic elements and compounds were discovered, and their industrial uses foreshadowed, during the course of the purely academic research work carried out in our universities and colleges; all have become the materials upon which great and lucrative industries have been built up. Although the scientific worker has certainly not exhibited any cupidity in the past—although he has been content to rejoice in his own contributions to knowledge, and to see great manufacturing enterprises founded upon his work—it is clear that the obligation revolves upon those who have reaped in the world's markets the fruit of scientific discovery to provide from their harvest the financial aid without which scientific research can not be continued.

The truth of this statement is well understood by those of our great industrial leaders who are engaged in translating the results of scientific research into technical practice. As evidence of this I may quote the magnificent donation of £210,000 by the British Oil Companies towards the endowment of the school of chemistry in the University of Cambridge, the noble bequest of the late Dr. Messel, one of the most enlightened of our technical chemists, for defraying the cost of scientific research, the gifts of the late Dr. Ludwig Mond towards the upkeep and expansion of the Royal Institution, one of the strongholds of British chemical research, and the financial support given by the Goldsmiths' and others of the great City of London Livery Companies (initiated largely by the late Sir Frederick Abel, Sir Frederick Bramwell, and Mr. George Matthey), to the foundation of the Imperial College of Science and Technology. The men who initiated these gifts have been themselves intimately associated with developments both in science and industry; they have understood that the field must be prepared before the crop can be reaped. Fortunately our great chemical industries are, for the most part, controlled and administered by men fully conversant with the mode in which technical progress and prosperity follow upon scientific achievement; and it is my pleasant duty to record that within the last few weeks the directors of one of our greatest chemical-manufacturing concerns have, with the consent of their shareholders, devoted £100,000 to research. Doubtless other chemical industries will in due course realize what they have to gain by an adequate appreciation of pure science.

If the effort now being made to establish a comprehensive

scheme for the resuscitation of chemical industry within our empire is to succeed, financial support on a very liberal scale must be forthcoming, from the industry itself, for the advancement of purely scientific research. This question has been treated recently in so able a fashion by Lord Moulton that nothing now remains but to await the results of his appeal for funds in aid of the advancement of pure science.

In order to prevent disappointment, and a possible reaction in the future, in those who endow pure research, it is necessary to give a word of warning. It must be remembered that the history of science abounds in illustrations of discoveries, regarded at the time as trivial, which have in after years become epoch-making.

In illustration I would cite Faraday's discovery of electromagnetic induction. He found that when a bar magnet was thrust into the core of a bobbin of insulated copper wire, whose terminals were connected with a galvanometer, a momentary current was produced; whilst on withdrawing the magnet a momentary reverse current occurred; a purely scientific experiment destined in later years to develop into the dynamo and with it the whole electrical industry. Another illustration may be given: Guyton de Morxeau, Northmore, Davy, Faraday and Cagniard Latour between 1800 and 1850 were engaged in liquefying many of the gases. Hydrogen, oxygen, nitrogen, marsh gas, carbon-monoxide, and nitric oxide, however, resisted all efforts, until the work of Joule and Andrews gave the clue to the causes of failure. Some thirty years later by careful application of the theoretical considerations all the gases were liquefied. The liquefaction of oxygen and nitrogen now forms the basis of a very large and important industry.

Such cases can be multiplied indefinitely in all branches of science.

Perhaps the most pressing need of the present day lies in the cultivation of a better understanding between our great masters of productive industry, the shareholders to whom they are in the first degree responsible, and our scientific workers; if, by reason of any turbidity of vision, our large manufacturing corporations fail to discern that, in their own interest, the financial support of purely scientific research should be one of their first cares, technical advance will slacken and other nations, adopting a more far-sighted policy, will forge ahead in science and technology. It should, I venture to think, be the bounden duty of every one who has at heart the aims and objects of the British Association to preach the doctrine that in closer

sympathy between all classes of productive labor, manual and intellectual, lies our only hope for the future. I can not do better than conclude by quoting the words of Pope, one of our most characteristically British poets:

> By mutual confidence and mutual aid
> Great deeds are done and great discoveries made.

WHERE DOES ZOOLOGY STAND?

By Professor J. STANLEY GARDINER

PRESIDENT OF THE ZOOLOGICAL SECTION

THE public has the right to consider and pass judgment on all that affects its civilization and advancement, and both of these largely depend on the position and advance of science. I ask its consideration of the science of zoology, whether or not it justifies its existence as such, and, if it does, what are its needs. It is at the parting of the ways. It either has to justify itself as a science or be altogether starved out by the new-found enthusiasm for chemistry and physics, due to the belief in their immediate application to industries.

It is a truism to point out that the recent developments in chemistry and physics depend, in the main, on the researches of men whose names are scarcely known to the public; this is equally true for all sciences. A list of past presidents of the Royal Society conveys nothing to the public compared with a list of captains of industry who, to do them justice, are the first to recognize that they owe their position and wealth to these scientists. These men of science are unknown to the public, not on account of the smallness of their discoveries, but rather on account of their magnitude, which makes them meaningless to the mass.

Great as have been the results in physical sciences applied to industry, the study of animal life can claim discoveries just as great. Their greatest value, however, lies not in the production of wealth, but rather in their broad applicability to human life. Man is an animal and he is subject to the same laws as other animals. He learns by the experience of his forebears, but he learns, also, by the consideration of other animals in relationship to their fellows and to the world at large. The whole idea of evolution, for instance, is of indescribable value; it permeates all life today; and yet Charles Darwin, whose researches did more than any others to establish its facts, is

too often only known to the public as "the man who said we came from monkeys."

Whilst first and foremost I would base my claim for the study of animal life on this consideration, we can not neglect the help it has given to the physical welfare of man's body. It is not out of place to draw attention to the manner in which pure zoological science has worked hand in hand with the science of medicine. Harvey's experimental discovery of the circulation of the blood laid the foundation for that real knowledge of the working of the human body which is at the basis of medicine; our experience of the history of its development gives us good grounds to hope that the work that is now being carried out by numerous researches under the term "experimental" will ultimately elevate the art of diagnosis into an exact science. Harvey's work, too, mostly on developing chicks, was the starting-point for our knowledge of human development and growth. Instances in medicine could be multiplied wherein clinical treatment has only been rendered possible by laborious research into the life histories of certain parasites preying often on man and other animals alternately. In this connection there seems reason at present for the belief that the great problem of medical science, cancer, will reach its solution from the zoological side. A pure zoologist has shown that typical cancer of the stomach of the rat can be produced by a parasitic threadworm (allied to that found in pork, Trichina), this having as a carrying host the American cockroach, brought over to the large warehouse of Copenhagen in sacks of sugar. Our attack on such parasites is only made effective by what we know of them in lower forms, which we can deal with at will. Millions of the best of our race owe their lives to the labors of forgotten men of science, who laid the foundations of our knowledge of the generations of insects and flat-worms, the modes of life of lice and ticks, and the physiology of such lowly creatures as *Amœba* and *Paramecium;* parasitic disease—malaria, Bilhaziasis, typhus, trench fever and dysentery—was as deadly a foe to us as was the Hun.

Of immense economic importance in the whole domain of domestic animals and plants was the rediscovery, early in the present century, of the completely forgotten work of Gregor Mendel on cross-breeding, made known to the pesent generation largely by the labors of a former president of this Association, who, true man of science, claims no credit for himself. We see results already in the few years that have elapsed in special breeds of wheat, in which have been combined with exactitude

the qualities man desires. The results are in the making—and this is true of all things in biology—but can any one doubt that the breeding of animals is becoming an exact science? We have got far, perhaps, but we want to get much further in our understanding of the laws governing human heredity; we have to establish immunity to disease. Without the purely scientific study of chromosomes (the bodies which carry the physical and mental characteristics of parents to children) we could have got nowhere, and to reach our goal we must know more of the various forces which in combination make up what we term life.

In agricultural sciences we are confronted with pests in half a dozen different groups of animals. We have often to discover which of two or more is the damaging form, and the difficulty is greater where the damage is due to association between plant and animal pests. Insects are, perhaps, the worst offenders, and our basal knowledge of them as living organisms—they can do no damage when dead, and perhaps pinned in our showcases—is due to Redi, Schwammerdam, and Réamur in the middle of the seventeenth century. Our present successful honey production is founded on the curiosity of these men in respect to the origin of life and the generations of insects. The fact that most of the dominant insects have a worm (caterpillar or maggot) stage of growth, often of far longer duration than that of the insects, has made systematic descriptive work on the relation of worm and insect of peculiar importance. I hesitate, however, to refer to catalogues in which perhaps a million different forms of adults and young are described. Nowadays we know, to a large degree, with what pests we deal and we are seeking remedies. We fumigate and we spray, spending millions of money, but the next remedy is in the use of free-living enemies or parasites to prey on the insect pests. The close correlation of anatomy with function is of use here in that life histories, whether parasitic, carnivorous, vegetarian, or saprophagous, can be foretold in fly maggots from the structure of the front part of their gut (pharynx); we know whether any maggot is a pest, is harmless, or is beneficial.

I won't disappoint those who expect me to refer more deeply to science in respect to fisheries, but its operations in this field are less known to the public at large. The opening up of our northwestern grounds and banks is due to the scientific curiosity of Wyville Thomson and his *confrères* as to the existence or non-existence of animal life in the deep sea. It was sheer desire for knowledge that attracted a host of inquirers to investigate the life history of river eels. The wonder of a fish

living in our shallowest pools and travelling two or three thousand miles to breed, very likely on the bottom in 2,000 fathoms, and subjected to pressures varying from 14 pounds to 2 tons per square inch, is peculiarly attractive. It shows its results in regular eel farming, the catching and transplantation of the baby eels out of the Severn into suitable waters, which can not, by the efforts of nature alone, be sure of their regular supply. Purely scientific observations on the life histories of flat fish—these were largely stimulated by the scentific curiosity induced by the views of Lamarck and Darwin as to the causes underlying their anatomical development—and on the feeding value and nature of Thisted Bredning and the Dogger Bank, led to the successful experiments on transplantation of young plaice to these grounds and the phenomenal growth results obtained, particularly on the latter. Who can doubt that this "movement of herds" is one of the first results to be applied in the farming of the North Sea as soon as the conservation of our fish supply becomes a question of necessity?

The abundance of mackerel is connected with the movements of Atlantic water into the British Channel and the North Sea, movements depending on complex astronomical, chemical, and physical conditions. They are further related to the food of the mackerel, smaller animal life which dwells only in these Atlantic waters. These depend, as indeed do all animals, on that living matter which possesses chlorophyll for its nutrition and which we call plant. In this case the plants are spores of algæ, diatoms, etc., and their abundance as food again depends on the amount of the light of the sun—the ultimate source, it might seem, of all life.

A method of ascertaining the age of fishes was sought purely to correlate age with growth in comparison with the growth of air-living vertebrates. This method was found in the rings of growth in the scales, and now the ascertaining of age-groups in herring shoals enables the Norwegian fisherman to know with certainty what possibilities and probabilities are before them in the forthcoming season. From the work on the blending together of Atlantic with Baltic and North Sea water off the Baltic Bight and of the subsequent movements of this Bank water, as it is termed, into the Swedish fiords can be understood, year by year, the Swedish herring fishery. It is interesting that these fisheries have been further correlated with cycles of sun spots, and also with longer cycles of lunar changes.

The mass of seemingly unproductive scientific inquiries undertaken by the United States Bureau of Fisheries, thirty to

fifty years ago, was the forerunner of their immense fish-hatching operations, whereby billions of fish eggs are stripped year by year and the fresh waters of that country made into an important source for the supply of food. The study of the growth stages of lobsters and crabs has resulted in sane regulations to protect the egg-carrying females, and in some keeping up of the supply in spite of the enormously increased demand. Lastly, the study of free-swimming larval stages in mollusca, stimulated immensely by their similarity to larval stages in worms and starfishes, has given rise to the establishment of a successful pearl-shell farm at Dongonab, in the Red Sea, and of numerous fresh-water mussel fisheries in the southern rivers of the United States, to supply small shirt buttons.

Fishery investigation was not originally directed to a more ambitious end than giving a reasonable answer to a question of the wisdom or unwisdom of compulsorily restricting commercial fishing, but it was soon found that this answer could not be obtained without the aid of pure zoology. The spread of trawling—and particularly the introduction of steam trawling during the last century—gave rise to grave fears that the stock of fish in home waters might be very seriously depleted by the use of new methods. We first required to know the life histories of the various trawled fish, and Sars and others told us that the eggs of the vast majority of the European marine food species were pelagic; in other words, that they floated, and thus could not be destroyed, as had been alleged. Trawl fishing might have to be regulated all the same, for there might be an insufficient number of parents to keep up the stock. It was clearly necessary to know the habits, movements, and distribution of the fishes, for all were not, throughout their life, or at all seasons, found on the grounds it was practicable to fish. A North Sea plaice of 12 in. in length, a quite moderate size, is usually five years old. The fact that of the female plaice captured in the White Sea, a virgin ground, the vast majority are mature, while less than half the plaice put upon our markets from certain parts of the southern North Sea in the years immediately before the war had ever spawned, is not only of great interest, but gives rise to grave fears as to the possibility of unrestricted fishing dangerously depleting the stock itself. There is, however, another group of ideas surrounding the question of getting the maximum amount of plaice-meat from the sea; it may be that the best size for catching is in reality below the smallest spawning size. I here merely emphasize that in the plaice we have an instance of an important food fish, whose

capture it will probably be necessary to regulate, and that in determining how best the stock may be conserved, what sizes should receive partial protection, on what grounds fish congregate and why, and in all the many cognate questions which arise, answers to either can only be given by the aid of zoological science.

But why multiply instances of the applications of zoology as a pure science to human affairs? Great results are asked for on every side of human activities. The zoologist, if he be given a chance to live and to hand on his knowledge and experience to a generation of pupils, can answer many of them. He is increasingly getting done with the collection of anatomical facts, and he is turning more and more to the why and how animals live. We may not know in our generation nor in many generations what life is, but we can know enough to control that life. The consideration of the fact that living matter and water are universally associated opens up high possibilities. The experimental reproduction of animals, without the interposition of the male, is immensely interesting; where it will lead no one can foretell. The association of growth with the acidity and alkalinity of the water is a matter of immediate practical importance, especially to fisheries. The probability of dissolved food material in sea and river water, independent of organized organic life and absorbable over the whole surfaces of animals, is clearly before us. It is possible that that dissolved material may be even now being created in nature without the assistance of organic life? The knowledge of the existence in food of vitamines, making digestible and usable what in food would otherwise be wasted, may well result in economics of food that will for generations prevent the necessity for the artificial restriction of populations. The parallel between these vitamines and something in sea-water may quite soon apply practically to the consideration of all life in the sea. Finally, what we know of the living matter of germ cells puts before us the not impossible hope that we may influence for the better the generations yet to come.

If it is the possibility in the unknown that makes a science, are there not enough possibilities here? Does zoology, with these problems before it, look like a decayed and worked-out science? Is it not worthy to be ranked with any other science, and is it not worthy of the highest support? Is it likely to show good value for the money spent upon it? Should we not demand for it a professorial chair in every university that wishes to be regarded as an educational institution? And has not the occu-

pant of such a chair a task at least equal in difficulty to that of the occupant of any other chair? Surely the zoologist may reasonably claim an equal position and pay to that of the devotee of any other science? The researcher is not a huckster and will not make this claim on his own behalf, but the occupant of this chair may be allowed to do so for him.

THE MAP OF EUROPE AFTER THE WAR

By JOHN McFARLANE, M.A.

PRESIDENT OF THE GEOGRAPHICAL SECTION

WHEN we turn to Austria we are confronted with the great tragedy in the reconstruction of Europe. Of that country it could once be said "Bella gerant alii, tu felix Austria nube," but today, when dynastic bonds have been loosened, the constituent parts of the great but heterogeneous empire which she thus built up have each gone its own way. And for that result Austria herself is to blame. She failed to realize that an empire such as hers could only be permanently retained on a basis of common political and economic interest. Instead of adopting such a policy, however, she exploited rather than developed the subject nationalities, and today their economic, no less than their political independence of her is vital to their existence. Thus it is that the Austrian capital, which occupies a situation unrivalled in Europe, and which before the war numbered over 2,000,000 souls, finds herself with her occupation gone. For the moment Vienna is not necessary either to Austria or to the so-called Succession States, and she will not be necessary to them until she again has definite functions to perform. I do not overlook the fact that Vienna is also an industrial city, and that it, as well as various other towns in Lower Austria, are at present unable to obtain either raw materials for their industries or foodstuffs for their inhabitants. But there are already indications that this state of affairs will shortly be ameliorated by economic treaties with the neighboring States. And what I am particularly concerned with is not the temporary but the permanent effects of the change which has taken place. The entire political re-orientation of Austria is necessary if she is to emerge successfully from her present trials, and such a re-orientation must be brought about with due regard to geographical and ethnical conditions. The two courses which are open to her lead in opposite directions. On

the one hand she may become a member of a Danubian confederation, on the other she may throw in her lot with the German people. The first would really imply an attempt to restore the economic position which she held before the war, but it is questionable whether it is either possible or expedient for her to make such an attempt. A Danubian confederation will inevitably be of slow growth, as it is only under the pressure of economic necessity that it will be joined by the various nationalities of southeastern Europe. The suggestions made by Mr. Asquith, Mr. Keynes, and others, for a compulsory free-trade union would, if carried into effect, be provocative of the most intense resentment among most, if not all, of the states concerned. But even if a Danubian confederation were established it does not follow that Austria would be able to play a part in it similar to that which she played in the Dual Monarchy. With the construction of new railways and the growth of new commercial centers it is probable that much of the trade with the southeast of Europe which formerly passed through Vienna will in future go to the east of that city. Even now Pressburg, or Bratislava, to give it the name by which it will hence be known, is rapidly developing at the expense alike of Vienna and Budapest. Finally, Austria has in the past shown little capacity to understand the Slav peoples, and in any case her position in what would primarily be a Slav confederation would be an invidious one. For these reasons we turn to the suggestion that Austria should enter the German Empire, which, both on geographical and on ethnical grounds, would appear to be her proper place. Geographically she is German, because the bulk of the territory left to her belongs either to the Alpine range or to the Alpine foreland. It is only when we reach the basin of Vienna that we leave the mid-world mountain system and look towards the southeast of Europe across the great Hungarian plain. Ethnically, of course, she is essentially German. Now although my argument hitherto has rather endeavored to show that the transfer of territory from one state to another on purely economic grounds is seldom to be justified, it is equally indefensible to argue that two states which are geographically and ethnically related are not to be allowed to unite their fortunes because it would be to their interest to do so. And that it would be to their interest there seems little doubt. Austria would still be able to derive some of her raw materials and foodstuffs from the Succession States, and she would have, in addition, a great German area in which she would find scope for her commercial and financial activities. Even if Naumann

were but playing the part of the Tempter, who said "All these things will I give thee if thou wilt fall down and worship me," he undoubtedly told the truth when he said:

> The whole of Germany is now more open to the Viennese crafts than ever before. The Viennese might make an artistic conquest extending to Hamburg and Danzig.

But not only would Austria find a market for her industrial products in Germany, she would become the great trading center between Germany and southeast Europe, and in that way would once more be, but in a newer and better sense than before, the *Ostmark* of the German people.

The absorption of Austria in Germany is opposed by France, mainly because she can not conceive that her great secular struggle with the people on the other side of the Rhine will ever come to an end, and she fears the addition of 6,500,000 to the population of her ancient enemy. But quite apart from the fact that Germany and Austria can not permanently be prevented from following a common destiny if they so desire, and apart from the fact that politically it is desirable they should do so with at least the tacit assent of the Allied Powers rather than in face of their avowed hostility, there are reasons for thinking that any danger to which France might be exposed by the additional man-power given to Germany would be more than compensated for by the altered political condition in Germany herself. Vienna would form an effective counterpoise to Berlin, and all the more so because she is a great geographical center, while Berlin is more or less a political creation. The South German people have never loved the latter city, and today they love her less than ever. In Vienna they would find not only a kindred civilization with which they would be in sympathy, but a political leadership to which they would readily give heed. In such a Germany, divided in its allegiance between Berlin and Vienna, Prussian animosity to France would be more or less neutralized. Nor would Germany suffer disproportionately to her gain, since in the intermingling of Northern efficiency with Southen culture she would find a remedy for much of the present discontents. When the time comes, and Austria seeks to ally herself with her kin, we hope that no impassable obstacle will be placed in her way.

The long and as yet unsettled controversy on the limits of the Italian Kingdom illustrates very well the difficulties which may arise when geographical and ethnical conditions are subordinated to considerations of military strategy, history, and senti-

ment in the determination of national boundaries. The annexation of the Alto Adige has been generally accepted as inevitable. It is true that the population is German, but here, as in Bohemia, geographical conditions appear to speak the final word. Strategically also the frontier is good, and will do much to allay Italian anxiety with regard to the future. Hence, although ethnical conditions are to some extent ignored, the settlement which has been made will probably be a lasting one.

On the east the natural frontier of Italy obviously runs across the uplands from some point near the eastern extremity of the Carnic Alps to the Adriatic. The pre-war frontier was unsatisfactory for one reason because it assigned to Austria the essentially Italian region of the lower Isonzo. But once the lowlands are left on the west the uplands which border them on the east, whether Alpine or Karst, mark the natural limits of the Italian Kingdom, and beyond a position on them for strategic reasons the Italians have no claims in this direction except what they can establish on ethnical grounds. To these, therefore, we turn. In Carniola the Slovenes are in a large majority, and in Gorizia they also form the bulk of the population. On the other hand, in the town and district of Trieste the Italians predominate, and they also form a solid block on the west coast of Istria, though the rest of that country is peopled mainly by Slovenes. It seems to follow, therefore, that the plains of the Isonzo, the district of Trieste, and the west coast of Istria, with as much of the neighboring upland as is necessary to secure their safety and communications, should be Italian and that the remainder should pass to the Jugo-Slavs. The so-called Wilson line, which runs from the neighborhood of Tarvis to the mouth of the Arsa, met these requirements fairly well, though it placed from 300,000 to 400,000 Jugo-Slavs under Italian rule, to less than 50,000 Italians, half of whom are in Fiume itself transferred to the Jugo-Slavs. Any additional territory must, by incorporating a larger alien element, be a source of weakness and not of strength to Italy. To Fiume the Italians have no claim beyond the fact that in the town itself they slightly outnumber the Croats, though in the double town of Fiume-Sushak there is a large Slav majority. Beyond the sentimental reasons which they urge in public, however, there is the economic argument, which, perhaps wisely, they keep in the background. So long as Trieste and Fiume belonged to the same empire the limits within which each operated were fairly well defined, but if Fiume become Jugo-Slav it will not only prove a serious rival to Trieste, but will prevent Italy

from exercising absolute control over much of the trade of Central Europe. For Trieste itself Italy has in truth little need, and the present condition of that city is eloquent testimony of the extent to which it depended for its prosperity upon the Austrian and German Empires. In the interests, then, not only of Jugo-Slavia but of Europe generally, Fiume must not become Italian, and the idea of constituting it a Free State might well be abandoned. Its development is more fully assured as the one great port of Jugo-Slavia than under any other form of government.

With regard to Italian claims in the Adriatic, little need be said. To the Dalmatian coast Italy has no right either on geographical or on ethnical grounds, and the possession of Pola, Valona, and some of the islands gives her all the strategic advantages which she has reason to demand. But, after all, the only danger which could threaten her in the Adiratic would come from Jugo-Slavia, and her best insurance against that danger would be an agreement by which the Adriatic should be neutralized. The destruction of the Austro-Hungarian fleet offers Italy a great opportunity of which she would do well to take advantage.

Of the prospects of Jugo-Slavia it is hard to speak with any feeling of certainty. With the exception of parts of Croatia-Slavonia and of Southern Hungary, the country is from the physical point of view essentially Balkan, and diversity rather than unity is its most pronounced characteristic. From this physical diversity there naturally results a diversity in outlook which might indeed be all to the good if the different parts of the country were linked together by a well-developed system of communication. Owing to the structure of the land, however, such a system will take long to complete.

Ethnic affinity forms the real basis of union, but whether that union implies unity is another matter. It is arguable that repulsion from the various peoples—Magyars, Turks, and Austrians—by whom they have been oppressed, rather than the attraction of kinship, is the force which has brought the Jugo-Slavs together. In any case the obstacles in the way of the growth of a strong national feeling are many. Serb, Croat, and Slovene, though they are all members of the Slav family, have each their distinctions and characteristics which political differences may tend to exaggerate rather than obliterate. In Serbian Macedonia, again, out of a total population of 1,100,000, there are 400,000 to 500,000 people who, though Slavs, are Bulgarian in their sympathies, and between Serb and Bulgarian

there will long be bitter enmity. Religious differences are not wanting. The Serbs belong to the Orthodox Church, but the Croats are Catholics, and in Bosnia there is a strong Mohammedan element. Cultural conditions show a wide range. The Macedonian Serb, who has but lately escaped from Turkish misrule, the untutored but independent Montenegrin, the Dalmatian, with his long traditions of Italian civilization, the Serb of the kingdom, a sturdy fighter but without great political insight, and the Croat and Slovene, whose intellectual superiority is generally admitted, all stand on different levels in the scale of civilization. To build up out of elements in many respects so diverse a common nationality without destroying what is best in each will be a long and laborious task. Economic conditions are not likely to be of much assistance. It is true that they are fairly uniform throughout Jugo-Slavia, and it is improbable that the economic interests of different regions will conflict to any great extent. On the other hand, since each region is more or less self-supporting, they will naturally unite into an economic whole less easily than if there had been greater diversity. What the future holds for Jugo-Slavia it is as yet impossible to say; but the country is one of great potentialities, and a long period of political rest might render possible the development of an important State.

This brings me to my conclusion. I have endeavored to consider the great changes which have been made in Europe not in regard to the extent to which they do or do not comply with the canons of boundary-making, for after all there are no frontiers in Europe which can in these days of modern warfare be considered as providing a sure defence, but in regard rather to the stability of the states concerned. A great experiment has been made in the new settlement of Europe, and an experiment which contains at least the germs of success. But in many ways it falls far short of perfection, and even if it were perfect it could not be permanent. The methods which ought to be adopted to render it more equable and to adapt it to changing needs it is not for us to discuss here. But as geographers engaged in the study of the ever-changing relations of man to his environment we can play an important part in the formation of that enlightened public opinion upon which alone a society of nations can be established.

THE ECONOMIC CONDITION OF EUROPE AFTER THE NAPOLEONIC WAR

By Dr. J. H. CLAPHAM, C.B.E., Litt.D.

PRESIDENT OF THE ECONOMIC SCIENCE AND STATISTICS SECTION

IN 1815 France had been engaged in almost continuous wars for twenty-three, England for twenty-two years. The German States had been at war less continuously; but they had been fought over, conquered, and occupied by the French. Prussia, for instance, was overthrown in 1806. When the final struggle against Napoleon began, in 1812, there was a French army of occupation of nearly 150,000 men in Prussia alone. From 1806 to 1814 Napoleon's attempt to exclude English trade from the Continent had led to the English blockade—with its striking resemblances to, and its striking differences from, the blockade of 1914–19. Warfare was less horribly intense, and so less economically destructive, than it has become in our day; but what it lacked in intensity it made up in duration.

Take, for instance, the loss of life. For England it was relatively small—because for us the wars were never people's wars. In France also it was relatively small in the earlier years, when armies of the old size were mainly employed. But under Napoleon it became enormous. Exact figures do not exist, but French statisticians are disposed to place the losses in the ten years that ended with Waterloo at fully 1,500,000. Some place them higher. As the population of France grew about 40 per cent. between 1805–15 and 1904–14, this would correspond to a loss of, say 2,100,000 on the population of 1914. The actual losses in 1914–18 are put at 1,370,000 killed and missing; and I believe these figures contain some colonial troops.

Or take the debts accumulated by victors and the requisitions or indemnities extorted from the vanquished. The wars of a century ago left the British debt at £848,000,000. According to our success or failure in securing repayment of loans made to Dominions and Allies, the Great War will have left us with a liability of from eight to nine times that amount. Whether our debt-carrying capacity is eight or nine times what it was a century ago may be doubted, and can not be accurately determined. But it is not, I would venture to say, less than six or seven time what it was, and it might well be more. A good deal depends on future price levels. At least the burdens are

comparable; and we understand better now where to look for broad shoulders to bear them.

After Waterloo France was called upon to pay a war indemnity of only £28,000,000, to be divided among all the victors. With this figure Prussia was thoroughly dissatisfied. Not, I think, without some reason. She reckoned that Napoleon had squeezed out of her alone, between 1806 and 1812, more than twice as much—a tremendous exaction, for she was in those days a very poor land of squires and peasants, whose treasury only received a few millions a year. England, who was mainly responsible—and that for sound political reasons—for the low figure demanded of France, found herself, the victor, in the curious position of being far more heavily burdened with debt than France, who had lost. England, of course, had acquired much colonial territory; but on the purely financial side the comparison between her and France was most unequal. England's total national debt in 1817 was £848,000,000. France's debt did not reach £200,000,000 until 1830.

The reasons why France came out of the wars so well financially were four. *First,* she had gone bankrupt during the Revolution, and had wiped out most of her old debt. *Second,* under Napoleon she had made war pay for itself, as the case of Prussia shows. *Third,* there was no financial operation known to the world in 1815 by which England's war debt, or even half of it, could have been transferred to France. *Fourth,* England never suggested any such transference, or, so far as I know, ever even discussed it.

France's financial comfort, immediately after her defeat, extended to her currency. During the Revolution she had made a classical experiment in the mismanagement of credit documents, with the assignats issued on the security of confiscated Church property; but after that she had put her currency in good order. Her final defeat in 1812–14, and again in 1815, did not seriously derange it. Indeed, the English currency was in worse order than the French, owing to the suspension of cash payments by the Bank of England; and so rapidly did France's credit recover after 1815 that in 1818 French 5 per cents. stood at almost exactly the present-day price of British 5 per cent. War Loan. That year she finished the payment of her war indemnity, and the last armies of occupation withdrew.

She had no doubt gained by waging war, and eventually suffering defeat, on foreign soil. No French city had been burnt like Moscow, stormed like Badajoz, or made the heart of a gigantic battle like Leipzig. Napoleon fought one brilliant

defensive campaign on French soil, in the valleys of the Marne and the Seine, in 1814. In 1815 his fate was decided in Belgium. Hardly a shot was fired in France; hardly a French cornfield was trampled down. But France, as in 1918, was terribly short of men, and, again, as in 1918, her means of communication had suffered. Napoleon's magnificent roads—he was among the greatest of road engineers—had gone out of repair; his great canal works had been suspended. These things, however, were soon set right by the government which followed him.

France's rapid recovery brings us to one of the essential differences between Western Europe a century ago and Western Europe to-day. In spite of Paris and her other great towns, the France of 1915 was a rural country, a land of peasants and small farmers. Only about 10 per cent. of her population lived in towns of 10,000 inhabitants or more. The town below 10,000, in all countries, is more often a rural market town, ultimately dependent on the prosperity of agriculture, than an industrial center. Parallels for France's condition must be sought to-day in Eastern Europe—in Serbia or Russia. It is a condition which makes the economics of demobilization easy. The young peasant goes back from the armies to relieve his father, his mother, and his sisters, who have kept the farm going. Moreover, France maintained a standing army of 240,000 men after 1815; and her losses in the Waterloo campaign had been so heavy that the actual numbers demobilized were relatively small. Demobilization left hardly a ripple of the surface of her economic life.

The German states were far more rural in character even than France. There were a few industrial districts, of a sort, in the West and in Saxony; a few trading towns of some size, like Hamburg and Frankfurt; but there was nowhere a city comparable to Paris. In 1819 the twenty-five cities which were to become in our day the greatest of the modern German Empire had not 1,250,000 inhabitants between them. Paris alone at that time had about 700,000. German statesmen, when peace came, were occupied not with problems arising from the situation of the urban wage-earner, though such problems existed, but with how to emancipate the peasants from the condition of semi-servility in which they had lived during the previous century. Here, too, demobilization presented few of the problems familiar to us. Probably not one man in ten demobilized was a pure wage-earner. The rest had links with the soil. The land, neglected during the war, was crying out for labor, and every

man had his place, even if it was a servile place, in rural society.

Things were different in England; but our demobilization problem was smaller than that of our Continental allies or enemies, who had mobilized national armies, though not of the modern size. On the other hand, we had kept an immense fleet in commission, the crews of which were rapidly discharged. Early in 1817 Lord Castlereagh stated in Parliament that 300,000 soldiers and sailors had been discharged since the peace. In proportion to population, that would be equivalent, for the whole United Kingdom, to nearly 750,000 to-day. For these men no provision whatever was made. They were simply thrown on the labor market; and the vast majority of them were ex-wage-earners or potential wage-earners, industrial, mercantile, or agricultural. The United Kingdom was not urbanized as it is to-day; but the census of 1821 showed that 21 per cent. of the population lived in cities of 20,000 inhabitants and upwards, and probably about 27 per cent. (as compared with France's 10 per cent.) lived in places of 10,000 and upwards. As industry in various forms, especially coal-mining, spinning, and weaving, was extensively carried on in rural or semi-rural districts, it is certain that at least one demobilized man of working age in every three was a potential wage-earner of industry or commerce. And as Great Britain had lost most of her peasant-holders, whether owners or small working farmers, the remainder of the demobilized rank and file were nearly all of the agricultural laborer class. They had to find employment; there was not a place in rural society waiting for them, as there was for the average French or German peasant soldier. It is not surprising that the years from 1815 to 1820 were, both economically and politically, probably the most wretched, difficult, and dangerous in modern English history.

Things were at their worst in 1816–17, both for England and for her continental neighbors. Western Europe was very near starvation. Had the harvest of 1815 not been excellent, so providing a carry-over of corn, or had the harvest of 1817 been much below the average, there must have been widespread disaster; so thorough and universal was the harvest failure of 1816. In the latter part of 1815 (December) wheat fell in England to 55*s.* 7*d.*, although no grain imports were allowed, except of oats. Early in 1816 the United Kingdom was actually exporting a little wheat. Then came a terrible spring—a long frost, snow lying about Edinburgh in May; all the rivers of Western Europe in flood. An equally disastrous summer followed. There was dearth, in places amounting to real famine,

everywhere—worst of all in Germany. Unlike France, the German states of a century ago were extraordinarily ill-provided with roads. What roads there were had gone to pieces in the wars. In winter even the mails could hardly get through with sixteen and twenty horses. Food supplies could not be moved over long distances by land; and the slightly more favored regions could not help the most unfortunate. There was a far wider gap between prices in Eastern and Western Germany in 1816 than there had been in the last bad famine year (1772). Each German state, in its anxiety, began to forbid export early in 1816, thus making things worse. At Frankfurt, the representatives of the German States, gathered for the Diet, could hardly feed their horses. Prices rose amazingly and quite irregularly, with the varying food conditions of the various provinces. In the spring of 1817 pallid half-starved people were wandering the fields, hunting for and grubbing up overlooked and rotten potatoes of the last year's crop.

In England the harvest failure of 1816 drove wheat up to 103*s*. 7*d*. a quarter for December of that year, and to 112*s*. 8*d*. for June of 1817. In Paris the June price in 1817 was equivalent to 122*s*. 5*d*. At Stuttgart the May price was equivalent to 138*s*. 7*d*. These are only samples. Think what these figures mean at a time when an English agricultural laborer's wage was about 9*s*. 6*d*., and a French or German unskilled wage far less. It must be recalled that there were no special currency causes of high prices either in France or Germany. These were real dearth prices. In the spring of '17 the French government was buying corn wherever it could find it—in England, North Africa, America—as another bad harvest was feared. Happily, the 1817 harvest was abundant, here and on the Continent. By September the Mark Lane price of wheat was 77*s*. 7*d*., and the Paris price 71*s*. 9*d*.

I have gone into price details for the purpose of drawing a contrast between a century ago and to-day. Except for the damage done to the German roads, the wars had very little to do with these food troubles of 1816–17. High and fluctuating food prices were the natural consequence of the general economic position of Western Europe a century ago. It was only in the most comfortable age in all history—the late nineteenth and early twentieth centuries—that low and stable food prices came to be regarded as normal. In the eighteenth century, when England fed herself and often had an exportable surplus, fluctuations were incessant. Take the ten years 1750–1760. The mean price of wheat at Eton in '52 was 45 per cent. above

the mean price in '50. The mean price in '57 was nearly 100 per cent. above the mean price of '50. On Lady Day '57 the price was 60*s.* 5¼*d.* On Lady Day '59 it was 37*s.* 4*d.* On Lady Day '61 it was 26*s.* 8*d.* The '61 mean price was exactly half the '57 mean price.

Eighteenth-century England was too well organized economically to be in much risk of actual famine, but for Ireland and large parts of the Continent famine was a normal risk. War and its effects had only accentuated, not created, that risk. Imports might reduce it, but could not avert it, because Western Europe tends to have approximately the same harvest conditions throughout, and it was impossible to draw really large supplementary supplies from anywhere else. So unimportant were overseas supplies that the Continent suffered very much more from the harvest failure of '16, in time of peace, than from the eight years' English blockade in time of war. If overseas supplies could be got they were hard to distribute, owing to defective transport facilities. Thanks to the work of the nineteenth century, the most terrific of all wars was required to bring Western Europe face to face with what had been both a war-time and a peace-time risk a century earlier.

THE STRENGTH OF MATERIALS IN AEROPLANE ENGINEERING

By Professor C. F. JENKIN, C.B.E., M.A.

PRESIDENT OF THE ENGINEERING SECTION

THE importance of research in all branches of industry is now becoming fully recognized. It is hardly necessary to point out the great possibilities of the Board of Scientific and Industrial Research, formed just before the war, or to lay stress on the attention which has been called to the need for research by events during the war. Probably in no branch of the Services was more research work done than in the Air Service, and the advances made in all directions in connection with flying were astonishing. My own work was confined to problems connected with materials of construction, and as a result of that work I have come to the conclusion that the time has come when the fundamental data on which the engineering theories of the strength and suitability of materials are based require thorough overhauling and revision. I believe that the present is a favorable time for this work, but I think that attention needs to be

drawn to it, lest research work is all diverted to the problems which attract more attention, owing to their being in the forefront of the advancing engineering knowledge, and lest the necessary drudgery is shirked in favor of the more exciting new discoveries.

It has been very remarkable how again and again in aeroplane engineering the problems to be solved have raised fundamental questions in the strength and properties of materials which had never been adequately solved. Some of these questions related to what may be termed theory, and some related to the physical properties of materials. I propose to-day to describe some of these problems, and to suggest the direction in which revision and extension of our fundamental theories and data are required and the lines on which research should be undertaken. Let us consider first one of the oldest materials of construction—timber. Timber was of prime importance in aircraft construction. The first peculiarity of this material which strikes us is that it is anisotropic. Its grain may be used to locate three principal axes—along the grain, radially across the grain, and tangentially across the grain. It is curious that there do not appear to be generally recognized terms for these three fundamental directions. A very few tests are sufficient to show that its strength is enormously greater along the grain than across it. How, then, is an engineer to calculate the strength of a wooden member? There is no theory, in a form available for the engineer, by which the strength of members made of an anisotropic material can be calculated.

I fancy I may be told that such a theory is not required—that experience shows that the ordinary theory is quite near enough. How utterly misleading such a statement is I will try to show by a few examples. Suppose a wooden tie or strut is cut from the tree obliquely so that the grain does not lie parallel to its length. In practice it is never possible to ensure that the grain is accurately parallel to the length of the member, and often the deviation is considerable. How much is the member weakened? This comparatively simple problem has been of immense importance in aeroplane construction, and, thanks to the researches made during the war, can be answered. The solution has thrown a flood of light on many failures which before were obscure. If the tensile strengths of a piece of timber are, say, 18,000 lb./sq. in. along the grain and 800 lb./sq. in. across it (radially or tangentially) and the shear strength is 900 lb./sq. in. along the grain—these figures correspond

roughly with the strengths of silver spruce—then if a tensile stress be applied at any angle to the grain the components of that stress in the principal directions must not exceed the above strengths, or failure will occur. Thus we can draw curves limiting the stress at any angle to the grain, and similar curves may be drawn for compression stresses. These theoretical curves have been checked experimentally, and the results of the tests confirm them closely except in one particular. The strengths at small inclination to the grain fall even faster than the theoretical curves would lead us to expect. The very rapid drop in strength for quite small deviations is most striking.

Similar curves have been prepared for tensile and compressive stresses inclined in each of the three principal planes for spruce, ash, walnut, and mahogany, so that the strengths of these timbers to resist forces in any direction can now be estimated reasonably accurately.

As a second example consider the strength of plywood. Plywood is the name given to wood built up of several thicknesses glued together with the grain in alternate thicknesses running along and across the plank. The result of this crossing of the grain is that the plywood has roughly equal strength along and across the plank. Plywood is generally built up of thin veneers, which are cut from the log by slicing them off as the log revolves in a lathe.

Owing to the taper in the trunk of the tree and to other irregularities in form, the grain in the veneer rarely runs parallel to the surface, but generally runs through the sheet at a more or less oblique angle. As a consequence the strength of plywood is very variable, and tests show that it is not possible to rely on its having more than half the strength it would have if the grain in the veneers were not oblique. It is therefore obviously possible to improve the manufacture enormously by using veneers *split off*, following the grain, in place of the present sliced veneers. The superiority of split or riven wood over cut wood has been recognized for ages. I believe all ladders and ladder rungs are riven. Hurdles, hoops, and laths are other examples. Knees in ships are chosen so that the grain follows the required outline.

Owing to the enormous difference in strength in timber along and across the grain, it is obviously important to get the grain in exactly the right direction to bear the loads it has to carry. The most perfect example I ever saw of building up a plywood structure to support all the loads on it was the frame of the German Schutte-Lanz airship, which was made entirely

of wood. At the complex junctions of the various girders and ties the wood, which was built up of very thin veneers—hardly thicker than plane shavings—layers were put on most ingeniously in the direction of every stress.

During the war I have had to reject numerous types of built-up struts intended for aeroplanes, because the grain of the wood was in the wrong direction to bear the load. The example shown—a McGruer strut—is one of the most elegant designs, using the grain correctly.

Many of the tests applied to timber are wrong in theory and consequently misleading. For example, the common method of determining Young's modulus for timber is to measure the elastic deflection of a beam loaded in the middle and to calculate the modulus by the ordinary theory, neglecting the deflection due to shear, which is legitimate in isotropic materials; but in timber the shear modulus is very small—for example, in spruce it is only about one sixtieth of Young's modulus—and consequently the shear deflection becomes quite appreciable, and the results obtained on test pieces of the common proportions lead to errors in the calculated Young's modulus of about 10 per cent.

The lantern plates show three standard tests; the first is supposed to give the shearing strength of the timber, but these test pieces fail by tension across the grain—not by shearing. Professor Robertson has shown that the true shear strength of spruce is about three times as great as the text-book figures, and has designed a test which gives fairly reliable results. The second figure represents a test intended to give the mean strength across the grain, but the concentration of stress at the grooves is so great that such test pieces fail under less than half the proper load. This fact was shown in a striking manner by narrowing a sample of this shape to half its width, when it actually bore a greater total load—*i. e.*, more than double the stress borne by the original sample. The third figure represents a test piece intended to measure the rather vague quality, "strength to resist splitting." The results actually depend on the tensile strength across the grain, on the elastic constants, and on the accidental position of the bottom of the groove relatively to the spring or autumn wood in the annular rings. Unless the theory is understood, rational tests can not be devised.

There are some valuable tropical timbers whose structure is far more complex than that of our ordinary northern woods. The grain in these timbers grows in alternating spirals—an arrangement which at first sight is almost incredible. The most

striking example of this type of wood I have seen is the Indian "Poon." The sample on the table has been split in a series of tangential planes at varying distances from the center of the tree, and it will be seen that the grain at one depth is growing in a right-hand spiral round the trunk; a little farther out it grows straight up the trunk; further out again it grows in a left-hand spiral, and this is repeated again and again, with a pitch of about two inches. The timber is strong and probably well adapted for use in large pieces—it somewhat resembles plywood—but it is doubtful whether it is safe in small pieces. No theory is yet available for estimating its strength, and very elaborate tests would be needed to determine its reliability in all positions. I had to reject it for aeroplanes during the war for want of accurate knowledge of its properties.

These examples show how necessary it is to have a theory for the strength of anisotropic materials before we can either understand the causes of their failure or make full use of their properties or even test them rationally.

The second material we shall consider is steel, and in dealing with it I do not wish to enter into any of the dozen or so burning questions which are so familiar to all metallurgists and engineers, but to call your attention to a few more fundamental questions. Steel is not strictly isotropic—but we may consider it to be so to-day. The first obvious question the engineer has to answer is, "What is its strength?" The usual tests give the ultimate strength, yield point, elastic limit, the elongation, the reduction of area, and perhaps the Brinell and Izod figures. On which of these figures is the dimension of an engine part, which is being designed, to be based? If we choose the ultimate strength we must divide it by a large factor of safety—a factor of ignorance. If we choose the yield point we must remember that none of the higher-grade steels have any yield point, and the nominal yield point depends on the fancy of the tester. This entirely imaginary point can not be used for accurate calculation except in a very few special cases. Can we base our calculation on the elongation—the reduction of area—the Izod test? If we face the question honestly we realize that there is no known connection between the test results and the stress we can safely call on the steel to bear. The only connecting link is that cloak for our ignorance—the factor of safety.

I feel confident that the only reliable property on which to base the strength of any engine part is the suitable *fatigue limit*. We have not yet reached the position of being able to specify this figure, but a considerable number of tests show that in

a wide range of steels (though there are some unexplained exceptions) the fatigue limit for equal $\pm$ stresses is a little under half the ultimate strength, and is independent of the elastic limit and nominal yield point, so that the ultimate strength may be replaced as the most reliable guide to true strength, with a factor—no longer of ignorance, but to give the fatigue limit—of a little over 2.

If the fatigue limit is accepted as the only sound basis for strength calculation for engine parts, and it is difficult to find any valid objection to it, then it is obvious that there is urgent need for extensive researches in fatigue, for the available data are most meager. The work is laborious, for there is not one fatigue limit, but a continuous series, as the signs and magnitudes of the stresses change. Many problems in connection with fatigue are of great importance and need much fuller investigation than they have so far received—*e. g.*, the effect of speed of testing; the effect of rest and heat treatment in restoring fatigued material; the effect of previous testing at higher or lower stresses on the apparent fatigue limit of a test piece. Some observers have found indications that the material may possibly be strengthened by subjecting it to an alternating stress below its fatigue limit, so that the results of fatigue tests may depend on whether the limit is approached by increasing the stress or by decreasing it.

Improved methods of testing are also needed—particularly methods which will give the results quickly. Stromeyer's method of measuring the first rise of temperature, which indicates that the fatigue limit is passed, as the alternating load is gradually increased, is most promising; it certainly will not give the true fatigue limit in all cases, for it has been shown by Bairstow that with some ranges of stress a finite extension occurs at the beginning of a test and then ceases, under stresses lower than the fatigue limit. But the fatigue limit in that case would not be a safe guide, for finite changes of shape are not permissible in most machines, so that in that case also Stromeyer's test may be exactly what is wanted. It can probably be simplified in detail and made practicable for commercial use. Better methods of testing in torsion are also urgently needed, none of those at present used being free from serious defects. Finally, there is a fascinating field for physical research in investigating the internal mechanism of fatigue failure.

JOHN TYNDALL (1820–1893)[1]

By Professor ARTHUR WHITMORE SMITH

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JOHN TYNDALL, British philosopher and physicist, was a most genial and interesting personality. In him a noble and generous nature was combined with a resolute will and lofty principles. He had a fine regard for the rights and feelings of others, and most of his controversies were in defense of truth and justice.

The great ideas of the conservation of energy and the mechanical equivalent of heat were novelties in his time, and his clear thinking and exposition did much to interpret the full significance of these laws. By the publication of his lectures in a style both clear and interesting, and expressed in non-technical language, he reached a large audience, and did more than any other person to secure the wide diffusion of these all-pervading truths that lie at the foundation of physical science.

In the Royal Society's catalogue of scientific papers 145 titles appear under Tyndall's name, and his more extensive writings comprise no less than 16 separate volumes. The complete story of a life so full can not be given in a single evening, and the mere reading of the titles of his many papers would occupy more time than is now allotted to me. But if we can catch a glimpse of his spirit, and gather a bit of inspiration from the enthusiasm with which he worked, this half hour will not be spent in vain.

John Tyndall was born near Carlow, in the southeastern part of Ireland, on the second of August, 1820. Originally of English descent, the Tyndalls crossed to Ireland in the seventeenth century. The elder John Tyndall, although poor, was a man of more than ordinary intellect, and he gave his son a good education in English and mathematics.

To a large extent, Tyndall was a self-made man. His mathematical training enabled him to enter the ordnance survey of Ireland at the age of nineteen, and because of his skill in drawing he was later selected for the English survey. For three years he was a civil engineer at Manchester, and during this

[1] An address delivered before the Research Club of the University of Michigan at a meeting commemorating the centenaries of John Tyndall and Herbert Spencer, 21 April, 1920.

time he spent much time in reading and in private study. It was partly through the reading of Carlyle that he was led to abandon the brilliant possibilities then open to a civil engineer, and devote his life to scientific study.

For some time he was connected with Queenwood College, in Hampshire County, where one of his duties was the instruction of a class in mathematics. His mind was ever on the alert to observe the natural phenomena of daily life, and his teaching was no mere following of text-book routine. It was his custom to give the boys their choice of following Euclid or trying problems of their own devising. The book was never chosen. Their diagrams were scratched on the walls and cut in the beams of the playground, thus showing the lively interest they took in the subject. They found it pleasant to prove by mathematics, and then verify by experiment, that the angular velocity of a reflected beam of light is twice that of the mirror from which it is reflected. And they were startled by the inference that if the earth turned seventeen times its actual speed all things at the equator would lose their weight and have the same tendency to fall upwards as downwards. The days spent with these boys made a deep impression upon Tyndall, and he looked forward to that future day when he might push these subjects a little further and add his own victories to the conquests already won.

The autumn of 1848 found him at Marburg, where, after two years of work, he received the degree of doctor of philosophy. His reputation as an investigator was established by the publication of his work on the magnetic properties of crystals, and the relation of magnetism and diamagnetism to molecular structure. The action of the atoms and molecules held an irresistible attraction for him and every investigation was conducted with molecular arrangement in mind. He was not satisfied with a few typical examples, but he examined nearly a hundred natural crystals and the entire collection of artificial crystals in the laboratory of Professor Bunsen. The subject was studied from every side until he had obtained a clear conception of all the conditions involved, and was able to formulate the general law.

Faraday had just published his researches on the behavior of crystals in taking a definite position when suspended between the poles of a strong magnet. "This force," he says, "appears to me to be very strange and striking in its character . . . for there is neither attraction nor repulsion."

It required long and patient effort to bring under the dominion of elementary principles the vast mass of facts which

experiments had brought to light, but the more he worked at the subject the more clearly did it appear to Tyndall that the action of crystals in the magnetic field was due, not to some new and unknown force, but to the modification of the known forces of magnetism and diamagnetism by the crystalline structure. It *was* true that the forces were neither attraction nor repulsion, taken singly, but it was *both*, thus producing a torque which turned the crystal into a determined position. The painstaking observations and the simply stated conclusion showed the qualities for which his work was ever distinguished.

Shortly after his return from Marburg he was appointed professor of physics in the Royal Institution, where Faraday was then director. Seldom have two men worked together so harmoniously as did Tyndall and Faraday during the years that followed. Their relationship from first to last was like that of father and son, and when Faraday died, fourteen years later, Tyndall succeeded him as director of the Royal Institution.

It was at this time also that he became acquainted with Spencer, who was about his own age, and with Huxley who was five years younger. This was the beginning of the most intimate of friendships. On all sorts of minor topics they were liable to differ in opinion, and they never hesitated to criticize each other; but the fundamental harmony between them was profound, for each cared immeasurably more for truth than for anything else. It was no small factor in his life for Tyndall to enjoy the friendship of these two men.

Not all of the investigations of Tyndall were carried out in the laboratory, for he was always awake to the events of daily experience. Even the "spirits" did not escape his observation, and it is especially interesting at this time to read of Tyndall's experience in this field.

The spirits themselves named the time and place of meeting, which proved to be a dinner at a private residence near London. The medium—a delicate looking young lady—was seated next to Tyndall. He records a bit of the conversation. He asked the young lady if she could see the curious things he had heard about—the light emitted by crystals, for example. "Oh, yes," she replied, "but I see light around all bodies."

T. "Even in perfect darkness?"

Med. "Yes; I see luminous atmospheres round all people. The atmosphere which surrounds Mr. C. would fill this room with light."

T. "You are aware of the effects ascribed to magnets?"

Med. "Yes; but a magnet makes me terribly ill."

T. "Am I to understand that, if this room were perfectly dark, you could tell whether it contained a magnet, without being informed of the fact?"

Med. "I should know of its presence on entering the room."

T. "How?"

Med. "I should be rendered instantly ill."

T. "How do you feel to-day?"

Med. "Particularly well; I have not been so well for months."

All the while there was in Tyndall's pocket, within six inches of her, a magnet; but he felt that nothing would be gained by showing it.

On the whole the evening was a dull one, but towards the end the spirits were asked to spell the name by which Tyndall was known in the spirit world. As the alphabet was slowly repeated a knock was heard when the letter P was reached. Beginning again, the letter O was knocked down. The next letter was E.

The knocks seemed to come from under the table, and Tyndall asked permission to go underneath to assure himself of the origin of the sounds. He remained under that table for a quarter of an hour, and was sure that no sound could be produced without his being able to locate its source. The spirits were urged and entreated to finish the word, but they had become dumb, and could spell no more. Tyndall then returned to his chair, but not without a feeling of despair regarding the prospects of humanity, never before experienced.

The spirits, however, resumed their spelling, and dubbed him, "Poet of Science." More than once after this he accepted invitations to be with the spirits. His comment is, "they do not improve on acquaintance. Surely no baser delusion ever obtained dominance over the weak mind of man."

In the autumn of 1854 Tyndall attended a meeting of the British Association at Liverpool, and at its close he took the opportunity to visit North Wales, where he saw the slate quarries. It interested him to see how readily the rock split open in parallel planes, like wood before an axe. The explanation that these were the layers in which the material was deposited did not satisfy him. Consultation with geologists showed him that the planes of cleavage were not those of stratification, and further investigation on numerous substances convinced him that the cleavage was caused by the effects of pressure.

Two years later these phenomena were made the subject of a lecture at the Royal Institution. His friend Huxley was

present, and suggested that this aspect of slaty cleavage might have some bearing on the laminated structure of glacier ice. They were both going to Switzerland that summer, and they arranged a joint excursion over some of the famous glaciers, where they could observe together the veined structure of the ice.

No man knows, when he commences the examination of a physical problem, where it will lead him. For Tyndall this was the beginning of many visits to the Alps, where he continued the study of glaciers for many summers. He satisfied himself that the veined structure was due to pressure, but he was especially interested in learning how a crystalline solid like ice could flow like a liquid. He pointed out the inadequacy of earlier theories, and showed by experimental demonstration that the flow was due to continued minute fracture and subsequent refreezing of the ice.

But once in Switzerland, the fascination of the mountains claimed him, and he became an Alpine enthusiast. Summer after summer he returned to conquer some untrodden Alp, or continue an unfinished investigation. The ascent of Mont Blanc was not complete without planting thermometers at several stations to record the cold of winter while he was absent. Nor was science alone to benefit from these excursions. The volumes that record his experiences are gems of literature, pervaded from cover to cover with the vigor and freshness of the Alpine air.

Tyndall always considered original investigation to be the great object of his life, and his most extensive researches are those in the domain of radiant heat. These experiments were stimulated by the conviction that not only the physical, but also the molecular, condition of bodies probably played a very important part in the phenomena of the radiation and absorption of heat. He wanted to show the physical significance of an atomic theory which had been founded on purely chemical considerations, and this object was continually kept in mind. Radiant heat was used as an instrument to explore molecular condition, and to bring clearly into view the astonishing change in physical properties when the atoms of simple gases unite to form more complex combinations.

As new advance often awaits the production of new instruments, so Tyndall's first requirement was a galvanometer of increased sensitiveness. Having made this galvanometer, and also a sensitive thermo-pile, he proceeded to examine the absorption of radiant heat by various gases. The gas to be examined

was placed in a long tube, closed at each end with windows of rock salt. The source of heat was a copper cube filled with hot water. The heat radiating from this copper box passed through the gas in the tube and fell upon the thermopile placed just outside the farther window. The other face of the thermo-pile was warmed from a second source, similar to the first. When the tube was empty, the galvanometer pointed to zero. When the tube was filled with gas, if the molecules possessed any power of intercepting the heat waves, that side of the thermo-pile would receive less heat, and the galvanometer would show a deflection corresponding to the amount of heat thus absorbed by the gas.

Examined in this manner, dry air, oxygen, nitrogen, and hydrogen showed a very slight absorption of the radiant heat. But when in chemical combination, the astonishing fact appeared that carbon dioxide absorbed nearly 1,000 times as much as dry air. Nitric oxide absorbed 1,600 times as much as either nitrogen or oxygen. And ammonia absorbed 5,500 times as much as either of its constituents. To make sure that these results were real, and that errors in the method of observation, or impurities in the gases used, did not mask the true effects, required some thousands of experiments. Observations were repeated again and again, and under various conditions, until he was thoroughly familiar with all the factors that could affect the result. And conclusions were not published until the experiments had convinced him that they were correct.

The case of aqueous vapor in the air proved so interesting and important that a special series of experiments was made upon it. Not only was the air of London examined, but to avoid possible errors due to the effect of local impurities, air was brought from the country and from the seashore. This air, containing the normal amount of aqueous vapor, absorbed 70 times as much heat as air from which the moisture had been removed.

The importance of these results are manifest when they are stated in a different way. It appears from this that the aqueous vapor which exists within ten feet of the earth's surface on a day of moderate humidity is sufficient to absorb 10 per cent. of the entire terrestrial radiation, a considerable portion of which is thus returned to the earth. He thus explained the burning heat by day, followed by the enormous chilling at night, in those places that are not protected by a blanket of moist air. In a general way, this has been referred to the purity of the air, but this purity consists in the absence of the transparent

vapor, rather than that of smoke or other visible constituent.

Thus a comparatively slight change in the variable constituents of the atmosphere, by permitting free access of solar heat to the earth and checking the outflow of terrestrial heat into space, might produce changes of climate as great as those which the discoveries of geology reveal.

An extension of this inquiry led him to investigate the absorption of heat by various liquids and their vapors. The behavior of these vapors placed them in a definite order of relative absorption. For heat of the same quality, and using equivalent amounts of the different liquids, he proved that the liquids occupied the same order as their vapors. This led him to the conclusion that the act of absorption is molecular, and that the molecule maintains its power as an absorber and radiator in spite of its change from liquid to vapor. Later he considered this action as due, in large part, to the atoms composing the molecule, rather than its being solely a property of the molecule as a whole.

In another series of experiments the source of radiant heat was a flame of burning gas. The radiation from the hydrogen flame possessed a peculiar interest, for he thought it likely that the resonance between its periods of vibration and those of the cool aqueous vapor of the air might be such as to cause the atmospheric vapor to exert a special absorbent power.

His surmise in this respect was justified, for he found that 20 per cent. of the total radiation from the hydrogen flame was absorbed by 50 inches of *undried* air, whereas only about 6 per cent. of the radiation from a hot platinum spiral was thus absorbed. Nor was this resonance confined to aqueous vapor. The dried air, which was now transparent to the hydrogen flame, was able to absorb 14 per cent. of the heat from a flame of carbon monoxide. Air from the lungs, with the moisture removed, was able to intercept 50 per cent. of the entire radiation from the carbon monoxide flame.

As a result of these investigations, carried out with extreme care and in great detail, he showed the very intimate relation between the absorption of heat and the molecular condition of the absorbing body.

In connection with these experiments, he also showed that those bodies that were the most efficient absorbers of radiant heat were likewise the best radiators of that same heat.

But while we are greatly indebted to Tyndall for the new knowledge which he discovered in the domains of heat and other branches of physics, his name will continue to be loved and re-

membered even more for the interesting lectures in which his methods of research were explained to public audiences, and the fundamental principles of science made clear to them.

It must have been a delight to listen to him. He was famed for the charm and animation of his language, for lucidity of exposition, and singular skill in devising and conducting experimental illustrations. Both he and his younger friend Huxley were popular with the London audiences, but they were very different. Huxley convinced his audience and compelled their assent; Tyndall carried them with him. They could not help agreeing with Huxley even if they did not wish to do so; they wished to agree with Tyndall if they could. It was the aim of Tyndall to rise to the level of his subject from a basis so elementary that every one in his audience could comprehend it, and then to lead them on by experimental demonstrations to a more complete understanding of the truths of Nature.

In the autumn of 1872 he came to America, where, in several of the eastern cities, he delivered a series of lectures on "Light." His success as a lecturer was complete. At first he was somewhat in doubt regarding the intellectual level that might be expected of the audiences, but he received early warning to talk the same as he would at the Royal Institution. One who heard him says: "It was a rare treat to hear him lecture. His illustrative experiments were beautifully done, his speech was easy and eloquent, and his manner, so frank and earnest and kindly, was extremely winning." His reception throughout was that of a friend by friends; and he looked back upon his visit as a memory without a single stain of unpleasantness.

The noble nature of the man and his unselfish devotion to the science he loved is shown by his attitude towards financial reward. This lecture season brought him about $13,000 over his actual expenses, but he would not take a cent of it. He left it all in the hands of trustees as a fund for the benefit of science in America. At the present time this fund is in the form of three graduate fellowships in physics—at Harvard, Columbia, and the University of Pennsylvania. It is of interest to recall that one of our own men has recently enjoyed one of these fellowships.

Tyndall, like most of his friends, was a reverent agnostic. He did not believe that the ultimate truths of the universe could be expressed in words, or that our limited and finite intelligence could as yet comprehend them. His writings, however, contain many phrases which show that he was familiar with the books of Holy Scripture. And often, after a Sunday evening tea, he would join his friends in the singing of psalm tunes.

On the question of miracles, he did not deny their possibility, but he compares, for example, the horse power involved in stopping the sun and moon (or was it merely the rotation of the earth?) with the feeble efforts expended by Joshua and his men in pursuing the five kings of the Amorites. And with characteristic consideration for the author, he points out that for him the sun was only a moving lantern, whose motion could be varied at the will of the appropriate authority.

His views on the great question of the relation between science and religion were expressed in his presidential address before the British Association at Belfast. In this address he outlined the fortunes of science from the times of the Greeks and the Moors, and depicted the struggle of truth against ignorance and superstition. Tracing back the theory of Darwin to the beginnings of life, he saw only the unbroken workings of Nature, extending beyond the range of experimental evidence, and this led him to the conclusion that the possibility of life must have existed in the atoms of the nebulæ.

He strongly maintained the claims of science to discuss such questions fully and freely in all their bearings, whatever the results might be. Such an address, delivered at the present time, would cause scarcely a ripple of dissent, but at that time it brought down on his head the severe criticism of those who differed from him, and a three days fast was proclaimed to keep infidelity out of Ireland.

An accident in the Alps may have been the cause that turned his mind to investigations in another direction. Having taken a shower bath under the cascade of an Alpine stream, he was returning for his clothes when he slipped and the sharp granite pierced his shin. Dipping his handkerchief in the clear water of the stream he bound up the wound and limped to his hut, where he lay quietly for several days. There was no pain, and upon removing the bandage the wound was found to be clean and uninflamed. But it soon became inflamed, and he had to be carried on men's shoulders to Geneva, where for six weeks he was confined to his bed.

About this time there was considerable dissension regarding the spontaneous generation of life, and Tyndall could not let such a question of fact pass by without adding his own clear logic to the discussion.

In the investigations on radiant heat it had been necessary to use air from which all traces of floating dust had been removed. A sensitive test of such purity was found in a concentrated beam of intense light, which rendered visible particles smaller than any microscope could detect.

Convinced that the reported cases of spontaneous generation of life were due to infection from the air, he wanted to try the effect of this optically pure air. Fifty wooden chambers were built, with windows on each side for the passage of a strong beam of light. When this showed that the air within was free from floating particles, various infusions of meat and vegetable were introduced in open test tubes and properly sterilized. There was no shade of uncertainty in any of the results. All of the infusions remained pure and sweet, although some of them remained freely exposed to the air in the chamber for over a year. Out of a total of 500 chances, there was no appearance of spontaneous generation. But when the air from the laboratory was allowed to enter the chambers, the infusions swarmed with life in two or three days.

Believing in the germ theory, and realizing that in certain stages of development the germs are more readily destroyed by heat, he devised the method of sterilization by repeated heating. In this method the infusion is brought to the boiling point, and then set aside for ten or twelve hours, after which it is brought to the boiling point again. Successive heatings in this manner destroyed the most resistant germs, three minutes of repeated boiling being more effective than 300 minutes of continuous heating.

In recognition of these researches he was given the degree of M.D. by the medical faculty of Tübingen.

At the age of 55 he married the charming and accomplished daughter of Lord Hamilton, whom he met during one of his Alpine excursions. They were companions in all things, living in his rooms at the Royal Institution, and spending their summers among his old haunts in the Alps. But his later years were marred by ill health and sleeplessness, and by accident, one evening in 1893 he took an overdose of chloral, from the effects of which he never awoke.

No other man had done more by research, lectures and writings, to discover and disseminate a sound knowledge of natural phenomena. And because there was no sacrifice of truth for popularity, the books he wrote half a century ago are classics at the present time.

GOVERNMENTAL RESEARCH[1]

By GEORGE K. BURGESS, Sc.D.

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AS an aftermath of war, the past two years have witnessed an unparalleled interest of world wide extent in the subject of scientific research, embracing its aims, scope and methods, as well as its relation to industry, university and government. A remarkable series of contributions and addresses, written mainly by leaders in, or directors of research, have called attention to the various aspects of the subject and have served to inform the public mind and stimulate it to a realization of the importance of research to the community on the one hand and the danger which attends ill considered plans on the other.

One of the most important forums for discussion of this fundamental subject has been the Royal Canadian Institute and I would like to recall, in this connection, particularly, the addresses before this Institute of Dr. George E. Hale on "Coöperation in Research" and Dr. Frank B. Jewett on "Industrial Research." It so happens that Messrs. Hale, Jewett and myself have been identified each with a separate and distinctive phase of the development of research; Dr. Hale with science unalloyed with industrial aspects or governmental control, Dr. Jewett with the applications of science to an industry, and the writer with scientific research in a government department. The preliminary training of each of us was remarkably similar; we are all graduates of the Masschusetts Institute of Technology, specializing in physical science and all had supplementary university training and some teaching experience, since which each has made his life's work in his chosen field of research of the three characteristic types, institutional, industrial, and governmental. We are all therefore exponents of the group method of carrying on research.

As a representative of this third type, that is, of research under government auspices, it may not be without interest to you to have from me a statement regarding the conduct of research in a Government Laboratory. I can of course do no better than give you the impression I have received from the development of the laboratories with which I am associated,

[1] Given at Royal Canadian Institute, April 24, 1920.

those of the Bureau of Standards, and especially in their relation to the public and the government in my own field of metallurgy.

I would first however call to your attention the tendency toward a somewhat different orientation of the relations of government to scientific research in the two countries that are most intimately related in blood and institutions to Canada, namely, Great Britain and the United States.

The relations of the British Government to research are set forth with great completeness in the annual reports of the Committee of the Privy Council for Scientific and Industrial Research and are ably summarized by Sir Frank Heath in an address before the society of Arts on "The Government and the Organization of Scientific Research."[2] The cardinal principles which have guided the development of this trust are set forth in Privy Council Committee's Report of 1918–19 and will bear repeating:

> We believe, in the first place, that while it is possible for the state by means of suitable grants to individuals or the generous support of universities and other independent institutions for research to encourage the pursuit of research in pure science, it is dangerous and even fatal to attempt to organize it. Research of this nature has no other aim than the creation of new knowledge and is impatient of the control which is inseparable from the idea of external organization. On the other hand, it is necessary for the modern State to organize research, including those simpler types of research which we may call investigation into problems which directly affect the well-being of large sections of its people. Such researches and investigation, deal either with applied science or if they are conducted in the realm of pure science are undertaken with a specific end in view. . . .
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> But in the second place, if the organization of research for public purposes is to be effective and economical which will be cognizant of the general lines of research undertaken by different departments of government, and a central body connected therewith capable of undertaking or organizing research which it is agreed can best be conducted by one agency in the interests of all.
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> In the third place, it is dangerous and even fatal under peace conditions for the state to attempt to conduct researches and investigations for the immediate benefit of industries which are not under state management. Industrial research is as integral a part of production or distribution as advertisement or insurance. But this does not preclude the state from encouraging the organization of research within an industry by means of grants-in-aid made under suitable conditions, or even by means of preliminary demonstrations of the valuable results which a well-conducted research may be expected to secure.
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> Finally, it would be fatal to the success of a department entrusted with the encouragement and organization of research to concern itself with exploitation or commercial development or administrative application of the results which may be obtained. . . .

[2] Sir Frank Heath, *Jl. Roy. Arts*, February 21, 1919.

It should be noted also that the English plan separates the administrative functions from the technical or scientific, the policies and progress of the latter being exclusively under the control of advisory committees composed of scientific or technical experts who are not members of the Department.

In the United State, the major branches of scientific research under governmental jurisdiction have not been gathered together under a single administrative head as in England but have been left each to the independent direction of the department of state in which it may be accidentally located, although there is now under consideration a measure that would group all the engineering and many of the scientific bureaus, except those dealing with agriculture, under a single Department of Public Works. The direction of scientific work, as at present organized, is almost exclusively in the hands of scientific men who combine the functions of an administrator and leader in research.

The American plan of independent administrative units does not mean that there are not often very intimate relations between scientific services in the various government departments including interchange of programs, partition of projects between them and other forms of cooperation maintained by a sufficiently close liaison, either formal or informal; although it could hardly be maintained that this common effort is as effective in all cases as could be wished. Again, certain scientific and technical services, such as the Advisory Committee for Aeronautics, are specifically constituted by law to include a membership from the several interested departments and the research work of this Committee is distributed among them. This method of bringing together the representatives of several branches of the service, some dealing with the application and others with the solutions of scientific problems in a definite field of research, makes for better understanding between theory and practice, and the scientific workers have the benefit of valuable advice from the men who are to use the results of the investigation. This constant interplay is beneficial to both. This method of joint research control could be extended with advantage to many other fields of the applications of science in which the government is interested.

One of the most important and fundamental fields of general interest, in which a beginning has been made to bring the departments together, is that of standardization and specifications. It has been proposed to make the Bureau of Standards a clearing house of information—to use the British term—on these subjects for the government. There are great possibilties here

for realizing economies in purchases and improvements in design and there will also result inevitably a considerable impetus to scientific research, especially on fundamental constants and properties of materials which form the basis of constructing standards and writing specifications. The formation of the American Engineering Standards Committee, in which the Departments of Commerce, Navy and War are represented and which is now working in a highly satisfactory manner, forms a further liaison with the public through the Engineering Societies, which again makes for development and improvement in many domains tributary to scientific research.

In looking over the multiplicity of research projects supported by the United States Government, one might easily get the impression that with such a widely scattered responsibility, as actually exists, for the planning of research in the various government departments, there might arise considerable confusion and duplication. A careful survey of the situation, however, would soon convince one of the surprisingly slight amount of overlapping in the scientific work among the several departments. I can speak with some positiveness on this subject as I have been recently occupied in such a survey as a member of the Board which has just completed a study in duplication in scientific work carried on by the government; and it is a fact that actual duplication is almost non-existent.

Although there are in the United States for certain lines of governmental scientific research, such formal or informal advisory technical committees as mentioned above, the rôle of initiation, correlation and stimulation of research, including industrial scientific research, taken in England by the Department of Scientific and Industrial Research, has been, in the United States, largely assumed within the past year or two by the National Research Council, the organization and scope of which has been so ably set forth before this Institute by its first Chairman, Dr. Hale.

The impetus given in England by the Government to the organization and support of Research Associations has been, it seems to me, one of the most remarkable of recent achievements in the successful intervention by a government for the encouragement of national industries by aiding in the formation of a type of organization by which the industries can best help themselves. The crucial test of this method of stimulating industrial research will in all probability come after the five years time limit of government support is reached when the Research Associations will have to shift for themselves.

The Research Council has been endeavoring to foster in the

United States the formation of somewhat similar cooperative scientific research associations among several of the American industries, but to this date, it would appear safe to say, with but indifferent success. It is perhaps, not too early to ask, why does the formation of such Research Associations readily succeed in Great Britain and apparently not in the United States? Is it because of government initiative and support that they are established so promptly in England and would they be eventually in the United States if such support were forthcoming? Is it, that in the United States the industries are already provided with all the research assistance they need or can make use of? Or, on the other hand, can it be argued our industrial leaders are not yet convinced of the value of this type of cooperative research? I do not venture to answer these questions, replies to which in the last analysis may be but a formulation of underlying national characteristics; but possibly you in Canada by your solution of the problem will help shed light that will aid us all.

What has been the policy of the Government of the United States toward scientific research and its applications to the industries of the country? To put the question is to call forth immediately, what is familiar to you all, the response that in many fields, notably in the agricultural sciences, the Government has been the most generous of sponsors. It is also supporting research in almost all domains of pure and applied science from astronomy and mathematics to metallurgy and road construction, and in many other branches than agricultural research it has been the pioneer and still is the leader. Moreover, in recent years, there has been a marked advance in governmental support of scientific research fundamental to industry, particularly as exemplified by the Bureau of Mines and of Standards.

So much has been written recently about the advantages, including atmosphere, surroundings and status of research conducted under university or institutional guidance on the one hand and in industrial establishments on the other hand, and so little has been said—and silence may appear to be more eloquent than speech under certain conditions—of the advantages of research under governmental auspices, that it is difficult to resist setting forth here some of the conditions, as I see them, of research in a government department and of the position of the scientific men in the government service.

If government service, which we must remember is service for the public, is so unattractive, as certain writers have intimated, why for instance has the senior scientific staff of the Bureau of Standards remained nearly intact from its founda-

tion nearly twenty years ago? There must be some other than pecuniary advantages to account for this stability of position among scientific leaders, which has been the rule, rather than the exception, until very recently in most of the scientific establishments of the American government. The present moment, marked by scientific men leaving the government in unprecedented numbers, may be accounted for primarily by the bidding for their services by industries and institutions that have been able to readjust their salary scales to meet the mounting cost of living more promptly than had the government. It is within reason to suppose however that this situation will be eventually readjusted.

What then are some of the advantages to the scientific man of his position in the government service as compared with the university man or the man in a research laboratory pertaining to industry, The attributes the research worker most cherishes are freedom for development within his chosen field; unhampered opportunity to publish the results of his discoveries; the stimulation afforded by the congenial atmosphere of sympathetic and critical co-workers; an absence of extraneous, irksome tasks; in the existence and maintenance of the ever changing material facilities for research. Taking the Bureau of Standards as a type of governmental institution devoted largely to scientific research, I can state from experience I know of no other type where these desirable attributes are more happily blended than here.

There is also the added satisfaction, or privilege if you will, the "government scientist" possesses, in that he is conscious of working directly for the public welfare in response to a public demand, expressed through the representatives of the people in Congress by their allotment of funds to support his work. This direct relation to the public—and it is much more intimate than many persons realize—gives him a pride and confidence in his accomplishments that cannot be had by any one working solely for himself and his science or for an industry or commercial firm. His sense of responsibility is enhanced and he will plan his work accordingly. As he demonstrates his ability to make efficient use of it, his freedom of choice of subjects is almost unlimited, and he has absolute liberty as to his methods of attacking the problems he sets out to solve. I wonder if more can be said for any other type of research center?

The craving to communicate his ideas and exhibit his work to others is a well-known trait of the scientific man. Among the hundreds, nay thousands of investigators in the industrial research laboratories the ideals of which have been outlined by

Messrs. Jewett, Mees,[3] Carty, Nutting, how many of these men have the opportunity of free communion with others? On all important problems—important from the technical, competitive point of view—absolute silence is usually the most rigid of pass words. What might become many able contributions to science never see the light of day, on account of, what appears to me, a misguided policy of secrecy which often extends to unessentials, from the manufacturer's point of view, in an industrial research laboratory. The following is an illustration among many: the director of a long-established industrial research laboratory showed me the other day the reports on a series of long since completed but as yet unpublished investigation of considerable general interest, two of which had just been duplicated and published by the Bureau of Standards where we had no knowledge of the previous work. In addition to the economic waste of unnecessary duplication, what is the effect on the morale of the men who did the work first and had it suppressed except for use in the plant?

The benefits of association and working in a community of considerable size where there may be rapid interchange and immediate availability of information and experimental facilities are often overlooked by those who advocate the advantages of research by lone individuals in the conditions of practical isolation often prevailing in even our larger universities. The laboratories of the government, and to a less extent the larger industrial laboratories, should be and unquestionably are able to secure more rapid progress and greater effectiveness in the execution of research than can the isolated worker.

Then as to the facilities or tools of research, the public laboratories, speaking generally, are better equipped than most private laboratories, although some of the industries maintain laboratories before which even the government laboratories pale. The industrial research laboratories to be effective must also possess as adjuncts development laboratories for manufacturing on an experimental scale. In pure science there are many problems, often the most fundamental such as the exact determination of physical constants and standards, which require very elaborate and costly layouts and often take a series of years for their completion. Such can best be left to the government laboratories.

It would thus appear that viewed from these various standpoints of freedom, publication, facilities, atmosphere, so dear to

[3] C. L. K. Mees, "The Organization of Industrial Scientific Research," McGraw Hill, 1920, contains an excellent bibliography of recent titles.

the research worker he is at least as well off in the government laboratory as elsewhere.

As an example of the operation of a government research laboratory in the United States, let us take the Division of Metallurgy of the Bureau of Standards. What does it do and how?

First as to organization; the Bureau is divided for administrative convenience into twelve divisions, the office, the plant, the shops, and nine scientific or technical divisions, each constituting one of the branches of scientific work carried out at the Bureau, electricity, optics, heat, chemistry, weights and measures, metallurgy, engineering physics, structural materials, ceramics. Each division is again divided into sections; thus in metallurgy there are sections of: (1) Microscopy and Structure of Metals, (2) Heat Treatment and Thermal Analysis, (3) Working and Miscellaneous Properties, (4) Chemical Metallurgy, (5) Foundry.

The methods of directing and conducting the research work within the division we may mention briefly. There are no rules and regulations. Funds are allotted to the Division either by direct appropriation of Congress for a specific purpose or from the general funds of the Bureau by the director. Each research is authorized by the director on the written advice of the division chief. At meetings of the leaders within the division the program of work is considered and as a result of these discussions supplemented by written estimates the divisional budget is made up. Frequent conferences of the leaders are held to determine questions of policy and the progress of the work is fully discussed. Occasional meetings of the separate sections are also held, and there are also constantly being held informal conferences of members of the staff interested in any problem. The whole Division meets once in two weeks when a formal presentation of some investigation is given by its author. Each member of the Division presents a monthly progress report in writing. The papers offered for publication are reviewed critically by a committee of experts within the Bureau. A personnel committee consisting of all chiefs of divisions passes on most promotions.

The supervision of the routine work of testing and standardization is carried out by the leaders in research covering the same subject. There may or may not be a distinction between the personnel engaged in testing and research depending upon circumstances. This arrangement makes for flexibility, and avoids indivious distinctions and has worked extremely well. Men showing an aptitude for research have the oppor-

tunity to show it even if they may be assigned originally to routine work, and conversely. The skeleton of the organization is however of little importance as compared with its spirit. The Bureau of Standards consists above all of men and women imbued with high ideals and is a living organism of the highest type.

The Division of Metallurgy was formed in July, 1913, and has grown in population from one to fifty-seven and has acquired a very complete equipment to meet the needs of metallurgical research and testing. Over the development of testing we have no immediate control. The public and the government departments send us what they will and we try to satisfy their demands. It is a remarkable fact worthy of note that whenever a new line of testing is announced, immediately there is set up a never-ceasing flow of materials or instruments for test, the volume of which is oftentimes embarrassing; and in consequence one of our most difficult problems is to adjust equitably our efforts as between the execution of tests, our routine work, and the carrying out of investigations, our preferred work of at least equal urgency. There is here of course the ever present danger of too easily choosing the immediate for the permanent. Although not so vociferously expressed the real demand for knowledge concerning fundamental constants and properties is at least as great as the need expressed in the polite but insistent requests for the report on a trivial test of a material of interest to but one party. We are obliged to remind ourselves at times that we are here to serve the best interests of the public in our several domains of science.

The field covered by our metallurgical researches and investigations embraces subjects confined mainly to what has been called products metallurgy as distinguished from what is called process metallurgy which latter is illustrated by the reduction of metals from their ores, the field of the Bureau of Mines.

As one of the subjects of metallurgical research which will undoubtedly have far-reaching consequences, mention may be made of the study of gases in steel, including the development of methods of analysis; the determination of the quantity and manner on inclusion of the several gases which may be present such as oxygen, nitrogen, hydrogen and the oxides of carbon; and the characteristic gas content for steels of different composition and as determined by the method of manufacture. An immediate application of the methods here employed has been developed in our investigation of steel welding methods and products. The clearing up of the behavior of welded metals as

influenced by its gas content will aid greatly in solving some of the difficult problems connected with the welding art.

We were greatly concerned during the war with the scarcity, real or threatened, of several minerals and metals of vital necessity to the industries of the country. Among these were manganese, tin and platinum, and it became necessary to modify manufacturing process and devise suitable substitutes. We did a great deal of research work along these lines. Thus in the case of tin, for example, which is all imported, we developed a satisfactory solder containing only ten per cent. of tin instead of the usual 40 or 50 per cent. This solder containing also 80 per cent. lead and 10 per cent. cadnium was as cheap as ordinary solder. The tin content of bearing metals for most uses, it was shown, could be very greatly reduced; and for the tin bronzes satisfactory alloys, if made of available metals, were substituted. In fact, I believe America could have carried on with some ten per cent. of the normal tin consumption.

A great deal of attention is given constantly to questions connected with the various types of failure of metals and metal products such as flaky steel and internal fissures, railroad materials and stress corrosion in structural bronzes, to mention but two types, and numerous papers on these subjects have been published by the staff. The various and puzzling aspects of the corrosion of metals also requires constant attention.

There is now under way a series of investigations on special steels including structural steels, high speed steels, and their substitutes, high chromium steels of various types, and of steels containing unusual elements.

Some of the other subjects of metallurgical research are copper crushes guages for testing powder, improvement of machine gun barrels to resist erosion, identification tags for the Army and Navy, spark plug electrodes, characteristics of bearing metals, metals for aeronautical instruments, centrifugal steel castings, comparison of ingot practice in steel manufacture, temperature control of metallurgical manufacturing operations, embrittling of the steel parts by cleaning, pickling and plating, standard test bars for various alloys, and many other matters.

I shall not tire you, however, with an enumeration, much less with a description, of each of the seventy odd research problems in metallurgy with which we are occupied. They may be found in summary form from year to year in the annual reports of the director and appear in detail as they are completed in the publications of the Bureau and the scientific press. I may mention, however, some of the broad lines along which we

are orienting our work and in doing so will endeavor to emphasize the cooperative aspects of this research work, for much of it is undertaken after consultation or in active participation with other groups having also an interest or a part in its accomplishment.

This cooperation in research takes several forms and is of various types; thus there may be one or more of the other departments of the government interested in the prosecution of a research in which we also have an interest. For example, there has been carried out an investigation of considerable magnitude on the development and properties of a series of special steels with a view to their serviceability for light armor; in that research the Bureaus of Mines, Standards and Navy Ordnance have participated. Again, in consultation with the Advisory Committee for Aeronautics a series of researches on light aluminum alloys have been carried out. For the Army Ordnance, and sometimes in cooperation with that establishment, a whole series of investigations have been executed or are still under way. The list of interdepartmental cooperative researches in metallurgy is of quite considerable length, but the above illustrations may suffice to show that to secure scientific coordination among the government departments a central body is not indispensable.

Turning now to our cooperative relations with non-governmental bodies our relations with some of the scientific and technical societies are very close. Thus the work of the American Society for Testing Materials is largely participated in by the Bureau and particularly in Metallurgy our work has often been oriented to meet the desires of the various technical committees which are planning important lines of research of interest to science and industry; for example, coated metals, corrosion of iron and steel, the standardization of ladle test ingots in steel making.

With the National Research Council and its several committees on metallurgical matters we are in most active cooperation; I need but mention the work of the Pyrometer Committee and the extraordinarily successful symposium on pyrometry held at the meeting last fall of the American Institute of Mining and Metallurgical Engineers.

There is still another type of cooperation that should be mentioned, namely, the solving in the government laboratory of some of the problems fundamental to manufacturing processes and standards which are of interest to an industry as a whole. The development of this type of cooperative industrial research is still in its infancy in the United States, and evidently requires

an experimental manufacturing plant for each type of industry. We have made some provision for this field of development in metallurgy by installing several operating or semi-manufacturing units which, on a quarter ton basis, will allow us to make any metal or alloy, submit it to various heat treatments, shape and work it by rolling, forging, or drawing.

I should like, before closing, to call your attention to a co-operative research of the greatest economic importance, conceived on a somewhat more comprehensive scale than anything else we have hitherto undertaken; I refer to the investigation just gotten under way under the auspices of a Joint Committee to study the effects of sulphur and phosphorous in steel and in the specifications for the various grades of steel. This is a subject about which there is a great deal of diverse opinion and the experimental results published thus far have not been considered of sufficient weight to justify changing the present and long accepted values of sulphur and phosphorous contents in steel by responsible specification making bodies.

The Joint Committee, the chairmanship of which is held by the Bureau of Standards, is constituted with representatives of the government including the Departments of Commerce, War and Navy, steel makers, and specification making bodies including the American Society for Testing Materials, the railroads, the automotive and shipping industries. It is hoped, in view of the fact that the program of tests is mapped out by unanimous agreement of all interested parties; the steel manufacture witnessed by representatives of all interests; and the tests carried out in government laboratories, that the results of this elaborate research will be determinative as to revision of the specifications in question.

With this summary review of a few of the aspects of governmental research, as I see them, and as illustrated specifically in the work in Metallurgy at the Bureau of Standards, I trust you will carry away the impression, which I have endeavored to convey, that there is a human side to research and that in the government service it is possible to be very close to the public, in fact a part of the public and not a group set apart to solve abstruse problems of little general interest. I believe it not only desirable but absolutely essential that a government laboratory, and by that term I mean the men who work in it, keep in closest possible touch with the professional as well as the nontechnical public it serves and that a crucial test of the usefulness of such a laboratory is the interest and above all the confidence it inspires.

THE MATHEMATICIAN, THE FARMER AND THE WEATHER

By THOMAS ARTHUR BLAIR

METEOROLOGIST, U. S. WEATHER BUREAU

IT may have been true when Mark Twain said it, "Everybody talks about the weather, but nobody does anything," but now-a-days the mathematicians are doing something. They are hitching the weather to the engine of a formula, measuring it with the yardstick of an equation, and weighing it in the balances of a co-efficient. They can tell how many million dollars a half inch of rain on the fifth of August will add to the corn crop of Ohio; how many additional automobiles the farmers can purchase as a result of a week of warm weather while the wheat heads are filling; and how much smaller the world's supply of cotton will be because of an August drought in Georgia.

Aspiring poets used to lament that all the possible figures of speech were long since exhausted, but the poets of to-day still find something new to say and new ways to say it. So the prosaic, practical scientists are saying something very new about three of the oldest subjects of human thought, weather, farming and mathematics. The weather is the oldest of them all as a basis of observation and remark; the practise of agriculture began early in the history of civilization, and the development of mathematics began soon after, notably among the Egyptians, and was carried to a high degree of excellence in some lines by the Greeks. Yet each of these is the subject of an extremely new and modern science. Meteorology, the science of the weather, is one of the newest of the sciences, and is yet in its infancy. Its beginnings date back to some observations made by Benjamin Franklin, but its application began a century later, just after the Civil War. Something of the growth of the modern science of agriculture, principally due to the work of the agricultural experiment stations, is known to all. In the old science of mathematics new theorems, processes and devices are constantly being developed. But now appears a group of men with an original idea. Knowing something of the modern aspects of each of these sciences, they are combining them and using the refined and elegant processes of mathematical statistics to determine the effect of various kinds of weather upon the

crops in their different stages of development, to ascertain the farmer's risk from unfavorable weather, and to find definite relations between weather happenings in different parts of the globe.

The mathematical processes are due largely to Professor Karl Pearson, of England, who has applied them primarily in the fields of biology and anthropology. In this country, the leader in the application of these methods to the problem of determining the influence of the weather on the crops is Professor J. Warren Smith, of the United States Weather Bureau, whose work in this line began in Ohio several years ago. For example, he has shown that the yield of corn in Ohio is very largely dependent upon the amount of rain in June, July and August. When the July rainfall is less than three inches, the average yield is 30 bushels per acre, when it is five inches or more, the yield is 38 bushels; which means that these two inches of rain have added 27,300,000 bushels to the corn crop of this State, worth at 1919 prices about $35,000,000. When the July rainfall is three and a quarter inches, the yield is 15,000,000 bushels greater than when it falls short of this amount by half an inch. Each quarter of an inch increase between the totals of two and four inches means an added value of about $7,800,000. Taking the four great corn-growing states of Indiana, Illinois, Iowa and Missouri, the addition of half an inch to a total of two and three quarters inches adds ten bushels per acre to the yield on the average. This thin layer of water is worth at present prices about $13 an acre, or a total for these four states of the Corn Belt of the significant sum of $4,000,000,000. Truly, if corn is king in this region, water is the power behind the throne.

But not content with this victory, the agricultural meteorologist advances to the next line of defense with the relentless weapons of statistical analysis, the machine guns of mathematics, and finds that the most important twenty-day period in Ohio is from July 21 to August 10; and finally goes over the top and locates the critical period in the first ten days of August. This is the time when the half inch or the quarter inch of rain is of the most value, and when you must have it if you are to get a big crop of corn. And this is the period immediately following the blossoming of the corn. Now, this idea of "critical periods" is new, the idea being that there are certain short periods of time in the growth of any crop during which its future prospects are largely determined, "a tide which, taken at the flood, leads on to fortune." In short, favorable weather at these times will produce a good crop and unfavorable weather

a poor one. In some crops this is a single short period; in some temperature is the most important, in others it is rainfall or sunshine.

Food is brought to the plant by the moisture in the soil and is converted into vegetable tissue by heat and by the direct action of the sun's rays. For every species of plant there are certain best temperature and moisture values, varying at different periods of growth. If these best values occur at the critical periods, excellent crops are certain, barring accidents. And here we arrive at a practical application; the climate of most places in the United States is pretty well known and completely exhibited in published tables. When tables of the critical periods of plant growth, together with the meteorological factors, whether temperature, rainfall or sunshine, most affecting growth at these times, likewise become available, we shall have but to compare the two sets of tables to determine whether a specific crop is climatically well adapted to a particular district. Further, there are ways of advancing or retarding, within certain limits, the time of occurrence of the critical periods, thus bringing them into the time when favorable weather is more likely to occur. This may be done by the use of an earlier or later variety, by varying the time of seeding, by cultivation, or by the use of fertilizers. Moreover, by cultivation, a quarter or even a half inch of moisture may be conserved, if the farmer knows just when it is most important to conserve it. If these methods fail, and the weather is still frequently unfavorable at critical periods, it will be necessary to substitute some other crop. In some cases these important periods are far enough ahead of harvest to enable increased attention to be given to other crops in the same year. For instance, the rainfall of May is the most important factor in the hay crop in most of the northern United States. If at the end of May the rainfall has been light, other forage crops may be planted to take the place of hay. The application of all this to farming under irrigation is obvious. In our arid and semi-arid west, the sun may be depended upon to supply an abundance of energy, and if just the right amount of water is applied at the right time, remarkable crops result. Hence the importance of knowing the right time.

I have referred principally to Professor Smith's study of the effect of the weather on the yield of corn in Ohio, but many other interesting results have been obtained, both by him and others, and both in this country and in Europe. Take wheat for example, one of the oldest and probably the most important of cultivated crops. In the growing of winter wheat, which is

exposed to all sorts of weather for nine months, through fall, winter, spring and summer, the weather of three or four ten-day periods in May and June is found to influence the crop to a much greater extent than that of all the rest of the time combined. These are the critical periods in the development of winter wheat, and they are associated with certain definite stages in the growth of the wheat plant. When the wheat is "jointing," that is, growing rapidly in height, cool weather is demanded, but later, while the heads are filling, it must be warm, and in between these periods, during the ten-days when the "boot" from which the head emerges is forming, dry weather is necessary for the best growth. There is indication also that cool weather is advantageous while the wheat is blossoming, and warm while it is ripening. In addition, a weight of evidence is accumulating that a heavy March snowfall is decidedly detrimental, contrary to the prevailing popular opinion.

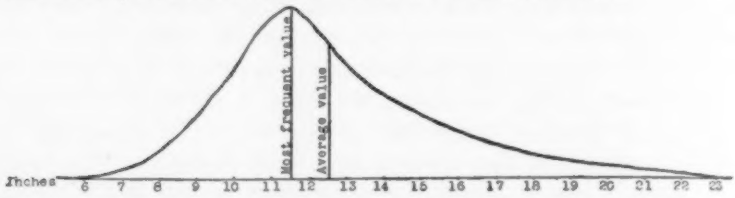
In the great spring wheat centers of North and South Dakota, it has been shown that the yield depends largely on the rainfall of May and June and the temperature of June, but no shorter critical periods have as yet been established. To obtain a large crop, the rainfall of May and June should be above the average and June should be cooler than the average. In North Dakota, which is the drier and cooler of the two states, a good rainfall is more important than cool weather, but in South Dakota temperature is in a great measure the determining factor, while in the neighboring State of Minnesota, variations in either temperature or precipitation during these months have little effect on the yield. No general rules for the entire country can be made, but each section must be studied with reference to its normal climate.

Consider, as another example, that staple of our dinner tables, the potato. A cool and wet July makes the potato crop in the Mississippi Valley. Cool weather is desirable all summer and wet weather during June, July and August, but July is the most important month and the first ten days of July the most important short period. This is the ten days following blossoming. If it is cool during this time with a good supply of moisture, and in addition the moisture supply has been fairly good during the previous two or three weeks, the prospects for a large crop of potatoes are excellent. If these conditions have not obtained, the yield will be small. If the water supply can be controlled by irrrigation, it is of the greatest importance that it be sufficient at this period.

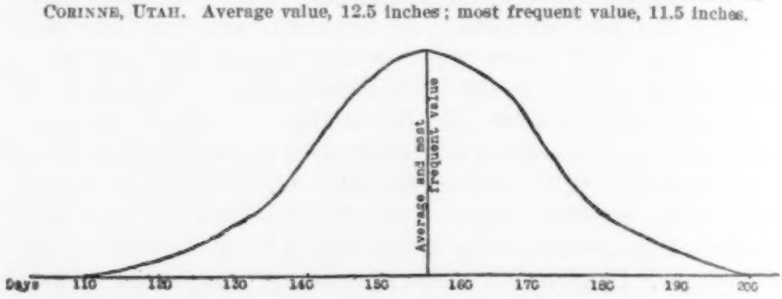
In the great Cotton Exchange in New York and in the primary cotton markets of the South the price of cotton has always fluctuated from day to day during the growing season with the daily reports of weather conditions in the Cotton Belt, where the world's supply of this staple is largely produced. But it has fluctuated erratically and without any solid knowledge of the exact amount of influence the weather may have upon the yield. Recently, however, a well-known American economist and statistician has shown that he can predict the total yield with remarkable accuracy by mathematical analysis from a knowledge of the average weather conditions from May to August, an accuracy greater than that of the estimates based on the condition figures of the Government crop reports. The favorable weather conditions differ somewhat in the different sections of the Cotton Belt, extending from Texas to South Carolina, but the most important requirements are that May shall be dry, June both warm and dry, and August cool and wet, a cool and wet August being of most importance. Sitting in his New York office, without even having seen a cotton plant growing and without receiving any reports as to the progress of the crop, the master of the newer statistics can at the end of August insert these weather values in his formula and tell how much cotton will be ginned in the South during the following autumn and winter. Such, in the hands of experts, is the magic in those bugbears of our school days, arithmetic and algebra!

There is another phase of weather, not directly connected with crop yields, towards which the powerful weapons of the mathematician have been directed. This may be called the application of frequency curves to climatic phenomena. A frequency curve offers a systematic method of examining the variations in a series of events, and, as applied to weather and climate, may be used to determine how often the summers will be too hot or too dry for a particular crop, or the winters too cold, or the growing season too short. The simple average, as usually given in climatic tables, is not sufficient. We must know how the individual years arrange themselves around the average. For, though these climatic events occur according to the laws of chance, they do not all follow the simple law by which, if you flip a coin a large number of times, heads and tails will appear with equal frequency. On the contrary, some of these happenings form "skew" curves; the rainfall, for example. In parts of the semiarid west an annual precipitation of twelve inches is considered sufficient for the growing of dry land grains, but that average will probably be made up of a few

years with much more than twelve inches and many years with amounts somewhat less than twelve. Though twelve is the average, it is not the most probable amount, and falls of less than twelve are more likely to occur than those of more than twelve. In such cases the distribution of events about the average is unsymmetrical, askew, and the average does not mean much, does not tell us what we want to know.



SKEW FREQUENCY CURVE SHOWING DISTRIBUTION OF ANNUAL PRECIPITATION AT CORINNE, UTAH. Average value, 12.5 inches; most frequent value, 11.5 inches.



SYMMETRICAL FREQUENCY CURVE SHOWING VARIATION IN LENGTH OF GROWING SEASON AT DENVER, COLORADO. Average and most frequent values coincide in 156 days.

The farmer or buyer wants to know not only what the average amount is, but how often in the course of ten or fifty years the amount will fall so far short of the average as to be entirely inadequate. This the makers of the frequency curve can tell him much more accurately than he could do for himself by simply counting the number of times it has been insufficient in the past ten or fifty years. In the northern portion of the orange-growing section of Florida, once in a good many years the trees are killed or badly frozen back by a winter cold wave. To know how often this is liable to occur is of prime importance in fixing the value of the land for orange-growing purposes. Similarly, in peach-growing sections, farther north, peach trees or the buds for next year's crop are subject to winter killing, and in nearly all fruit-growing sections the crops are liable to injury by late spring frosts. In early vegetable farming, the farmer frequently wants to take the risk of having his crops killed once in five or ten years in order to be in the market early in the other years. Is it better for him to go it

blindly, depending on his own impression of the proper date of planting, or to rely on the theoretical determination of the risk, which in 73 per cent. of the cases will lead to no unexpected losses, and in 94 per cent. to not more than one such loss in a period of twenty to thirty years?

In such cases as these the object is to determine the average interval between the occurrence of certain unfavorable conditions, such as insufficient rain, late spring frosts, early autumn frosts and other adverse events. This is the question that is answered by these curves, for by a little additional calculation the "frequency" curve becomes an "average interval" curve. By the use of these devices of the mathematician, it becomes possible from the examination of a limited number of observations to obtain a reasonable estimate of events as they will occur in an unlimited series of observations and hence to predict what is going to happen on the average in the next 20 or 100 or 1,000 years. Of course, it is not possible to tell by these means, nor by any others now known, just when such unfavorable events will happen. They may occur in two successive years and not again for 20 years. But, though they appear to happen fortuitously, in the long run they will occur the number of times indicated by the curve, and it is the performance of the land and the weather in the long run that determines values, though to the individual farmer the events of a few specific years may be of first importance.

The application of the statistical method to this individual phase of the problem, in what has been called "weather insurance," offers a legitimate opportunity for an extension of the field covered by insurance companies. We now have marine insurance, which includes perils of the sea due to storms, also hail and tornado insurance in certain parts of the country, but the idea may be greatly extended. Basing the work upon the methods I have described and upon the accurate climatological data collected by the Weather Bureau, there should be a statistical determination of the farmers' many risks from unfavorable weather conditions. Then the proper charge against the weather hazard can be made, and unseasonable and unusual weather will cease to be a calamity to the individual, just as the financial losses by fire and death are minimized in fire and life insurance, and the burden which is at present carried by individual losses and by depreciation of land values will be more widely distributed.

With such insurance well established it will be applicable in a wider field than the distribution of the individual risk. The

insurance rate quoted on a farm will give the purchaser valuable information. The country banker and storekeeper, who frequently carry the farmer through bad years, will be able to insure themselves against a great drain upon their resources in any one year. Instead of the haphazard, unbusinesslike method of taking unknown chances, which characterizes much of the present practice, the weather becomes a determinate risk in farming, a risk that can be stated more easily and more accurately than most other business risks.

Turning now from the numerous climatic problems of the agriculturist to those even more extensive fields of investigation, the physics of the atmosphere as a whole and the interrelations of its various parts, we find that the mathematical weather men have brought to light another series of interesting and curious facts. When an English scientist announces that it will be warm in Cairo, Egypt, to-morrow, since it was cold in London to-day, and that the rainfall will be unusually heavy in England this winter, since it was unusually light in Cuba last summer; when another says that a light rainfall in Chile during the period from May to August will be followed during July to October by more than ordinary floods on the Nile; when a Japanese mathematician says that the rice crop in northern Japan will be large this fall, since the barometer was unusually high last spring over China; when the scientists begin making such long range and curiously disconnected forecasts as these, it would seem that they are beginning to understand something of how this complicated atmosphere of ours works. As a matter of fact, they are conservative, and do not make any such forecasts for individual periods, but they have shown that on the average and to a great extent such relations do hold.

Many such correspondences between weather happenings in widely separated parts of the world have been shown. The rainfall of the central United States shows a direct correspondence to that of central South America, and both show an inverse relation to the rainfall of Australia. A forecast of the temperature at Berlin in March and April is possible at the end of December from the temperature at Christiania, Norway. When the April temperature at Irkutsk, Siberia, is higher than the normal, we may expect with a high degree of probability that the temperature at San Francisco in the following July will be abnormally low, and conversely. The higher the barometric pressure in the Argentine and Chile during March and April the greater will be the monsoon rainfall of India the following July and August. Florida and southern California refuse to pull

together in the matter of weather. Especially in the winter months, when one is warmer than normal, the other insists upon being cooler. Starting with San Diego as a basis of comparison and moving east and north, we find that the temperatures show a decreasing correspondence to those at San Diego, until, in the Mississippi Valley, the relation changes from positive to negative, and the eastern part of the country generally has temperature conditions opposite to those in southern California, culminating at Jacksonville, Florida, in a high degree of contrariness. Thus, it is fortunate for the orange market that freezes are not likely to occur in Florida and California in the same winter.

Such are some of the curious but apparently unimportant facts which have been revealed by the application of these novel methods of investigation to the study of climatic data. They are evidence that our entire atmosphere functions more or less as a unit, and though some of the results may seem at first sight to be of little moment, they promise in the end to prove of the greatest value. Their significance lies in the fact that they are leading toward an understanding of those great motions and shiftings of the atmosphere which cause our changeful weather, and make one winter to differ from another winter in severity as one summer differeth from another summer in torridity. A thorough understanding of these movements should, in time, lead to the solution of that fascinating problem of the climatic forecast, now the realm of charlatans, but the dream of real scientists, which aims at predicting the general character of a season months in advance. When that time comes, the mysteries of the weather largely will have vanished, and the meteorologist may say with the Wise Man in Yeats's play, "I have made formations of battle with Arithmetic that have put the hosts of heaven to the rout."

ANCIENT BACTERIA AND THE BEGINNINGS OF DISEASE

By Professor ROY L. MOODIE
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GERMS are among the oldest inhabitants of the earth. It is even suggested that while the earth was still forming bacteria were carried from distant planets on meteorites and thus initiated life on the earth. However this may be, bacteria are found in the oldest fossil-bearing rocks of North America, having been discovered by Dr. Charles Walcott in the central portion of Montana in association with fossil algæ, in the substance of which the bacteria were fossilized. Far from being disease-producing these earliest types of bacteria were doubtless of the kind which assist in withdrawing calcium from the sea water. They were rock builders. An analogous form exists in the Atlantic Ocean at the present day, and is especially active around the West Indies in building up the coral reefs.

The form of these most ancient germs is so similar to that of recent bacteria that they are called *Micrococcus*, a bacterial form which is especially common to-day. Considerable comment has been aroused as to the possibility of such delicate organisms as bacteria being capable of preservation in a fossilized condition. This is, however, pretty definitely settled by investigations in other lines. Fossil brains, fossil flowers, fossil blood and fossil muscle are known to be so well preserved that there is permitted an examination of the minute structure of the tissues. Renault, too, has described a great number of bacteria in the coal of France so no doubt exists longer as to the structures seen being bacteria.

Disease, however, did not exist with the most ancient bacteria. They were harmless, as are most of the present-day bacteria. Whether bacterial organisms were instrumental in effecting the origin of disease we do not know. This is a wide field of study which has not yet been explored. In a later geological period bacteria are found in partially decayed bone, together with thread mould and other types of fungi. This condition, however, can not be regarded as disease, but decay in dead material. The earliest animals were free from disease, although they were subject to injuries incident to the life of any

creature. The larger attacked the smaller then as now. Infection of the injured part did not take place in the early periods of animal life, and it is only after the great Coal Period that infected wounds are found.

The Coal Period witnessed the earliest widespread condition of bacteria and fungi, and possibly witnessed the beginning of disease, although there had been previously a mild form of pathology due to the action of parasites. The first diseased conditions preserved are, of course, not the earliest manifestation of disease, since disease is doubtless the result of long ages of struggle between the two contending forces of nature. The early animals were so highly immune to attack by bacterial organisms that it was only after the races of animals began to grow weaker through age that disease was able to make any headway.

It is idle to attempt to place a beginning of any limited time during which disease began. Disease was not present in the earliest times of the earth's history and it did not become very active until the present age of the earth had been attained by nearly three quarters of its duration. That is, disease has only been active during the last one quarter of the earth's history, so far as animals and plants are concerned. The incidence of maladies began slowly, was introduced gradually, and has been an important factor only within relatively recent times. It was a minor and unimportant factor for millions of years.

The action of early parasites on the shells of ancient animals are our oldest evidences of disease. The action of these organisms resulted in the formation of the oldest tumors. Diseased conditions of a very interesting type were caused in the early history of animal life by poisoning of the waters in which the animals lived. This resulted in a thickening of the shell, a twisting of the spirals of snails, or a diminution in size of some forms, certain of the depauperized individuals being only one twentieth their normal size.

The origin and development of disease may be traced to a large extent from the evidences of pathology found on the fossil bones of the ancient races of man and extinct animals, as well as from the associations of the earliest animals. That early man may have acquired some of his diseases from the coexisting animals is evident from the fact that the men of the stone ages, the cave bears, and other cave-inhabiting animals were often afflicted with the same maladies, as may be seen from the diseased appearance of their bones.

It would thus seem that the relation between disease in ancient times and the extinction of great groups of animals like the dinosaurs, was a matter of minor importance. The indications of disease so far seen on ancient bones are the results of accidents, or minor constitutional disturbances which did not endanger the life of the race and seldom that of the individual. The evidence, to be sure, is scanty, being confined to that seen on the hard parts of ancient animals. But on a similar basis is erected our present extensive knowledge of the evolution of animals in past time. Many of the epidemic diseases of to-day which are so fatal to life leave no traces on the bones. It may have been so in past times, to a great extent.

The beginnings of disease are thus seen to be lost in an immense obscurity of time during which the evil forces of nature were battling with the good for supremacy. Immunity doubtless was early established, and strongly entrenched. So firmly guarded were the primitive animals of the first ages of the earth that no disturbing influences entered into their existence. Only when racial old age, and the introduction of other antagonistic influences disturbed this natural immunity did animals see the new factor of disease enter into their lives. Early land animals doubtless lived long lives of placid contentment undisturbed by fear of infection either from within or without. Disease was in its very beginnings and with the land animals spread more and more over the face of the earth as time passed on in a mighty succession of geological ages.

ZOOLOGY IN THE A. E. F.

By Dr. ROBERT T. HANCE

ZOOLOGICAL LABORATORY, UNIVERSITY OF PENNSYLVANIA

THE sudden halt in the war left approximately two million Americans stranded in Europe with the chief object of their exile and with the stimulus and the real need for work gone. It was quite obvious, whatever the temporary belief of those prevented from returning, that it would take some time to reverse the concentrated efforts to build up a mighty force in France and return the individuals to their homes. Marking time with no point in view would shortly bring discontent and disorder.

The gathering together of the large group of young men from the entire country and their subsequent examination had shown an almost unbelievable lack of uniform or satisfactory education. Indeed in far too many cases schooling had never been a factor in the individuals' lives. Clearly such conditions are not to the advantage of the nation. It was then with the intention of keeping the men engaged in work of the greatest interest not only to the nation but to themselves that the Army Educational system of the American Expeditionary Force was started. The opening paragraph of the orders (G. H. Q., A. E. F., G. O. No. 30) authorizing the opening and equipping of these schools expresses the view of the responsible officers.

> 1. The commander in chief invites the attention of organization commanders and of all officers in the American Expeditionary Forces to the importance of national education. The citizen army must return to the United States prepared to take an active and intelligent part in the future progress of our country. Educational and occupational training should therefore be provided to meet the needs of the members of the American Expeditionary Force in order that they may be better equipped for their future responsibilities.

Beyond the immediate occupation of the men in useful work the directors had in mind an experiment in national education the results of which might be of value in developing a similar plan in this country.

All grades of education were provided from primary to the highest type of academic teaching. It is with the latter that the present paper will chiefly deal. French and British uni-

versities opened their halls to Americans (chiefly college graduates) whose training fitted them for the work given but the greater need was for the continuation of the training of those soldiers whose higher professional education had been interrupted by the war. To meet this need the American E. F. University was founded at Beaune, Cote D'Or, France, a quite charming town whose inhabitants did much to make the sojourn of the Americans a thing to be remembered.

The near-by forest-covered hills and vineyards made (when the rains stopped) an exceedingly picturesque background or campus and possibilities of beautiful walks were well realized. It is the purpose of the following article to attempt to picture the development of an American educational institution of collegiate rank on foreign soil with a staff and equipment drawn almost exclusively from the army. This can be done most satisfactorily by considering the conditions met chiefly by the department with which the writer was connected and with which naturally he was most familiar, namely, the department of zoology. Beaune had been selected as the site for the university because of the location just without the town limits of an American Hospital Center which had been built in anticipation of the great drive which the Armistice fortunately prevented.

Those who arrived in February shortly after the first group had taken up their duties at Beaune found the situation far from promising. The steady rains, the poorly developed and muddy roads through the camp and the apparent general lack of preparedness were sufficient to dampen any one's enthusiasm. The complaints of many who had been ordered to teach when expecting orders home were couched in terms more generally used in the American Expeditionary Force than in America. Some were disgusted and some insulted by the conditions and state in which they found themselves. They didn't believe that much could result from Army education. One officer arrived seething after having been asked by telephone if he would come and had understood that the invitation was to give a course of lectures at the Sorbonne. The latter setting rather pleased his fancy and would look well in print at home and the collapse following the receipt of orders and the realization that his lectures must be presented far from Paris did not tend to sweeten his temper. It was frequently postulated and with some reason that the soldier student who came would be actuated more by the relief from the tedium of drill than the opportunity offered for education and that consequently the teachers would be wasting their time.

Despite pessimism operations moved ahead with remarkable speed. The draughting rooms were busy day and night drawing and blue printing plans for the subdivision of the buildings according to the desires of the departmental heads, the engineers were carrying out the work rapidly and well (the partitions being of heavy cardboard), labor gangs were turning the roads into something worthy of the name and the faculty was threshing out policy, organization, courses, ordering books and all the rest of the academic detail. Literally a complete university was to be raised from the mud and this in a hurry. Everything from buildings to equipment had to be developed in the minimum time and instructors had to be found somewhere in the American Expeditionary Force. In the interesting way in which in Army affairs the right men for certain places gradually come along a rather well balanced teaching staff was soon assembled. Illustrative of the training and experience of the faculty in general was the group that finally gathered together to teach zoology.

A. H. Bayer, B.S. (Michigan Agricultural College), private, assistant in zoology in the American E. F. University.

Wendell Lowell Bevan, B.S. (Agricultural College of Colorado), captain Field Artillery, instructor in the Agricultural College of Colorado and instructor in entomology in the American E. F. University.

Robert T. Hance, M.A., Ph.D. (University of Pennsylvania), first lieutenant Sanitary Corps, assistant in zoology in the University of Pennsylvania and chairman of the Department of Zoology and instructor in genetics and microscopical technique in the American E. F. University.

Hovey Jordan, M.S. (Harvard) first lieutenant infantry, Austin fellow Harvard Graduate School. Instructor in histology and embryology in the American E. F. University.

Homer O. Moser, B.A. (Bluffton College, Ohio), sergeant, Medical Corps, instructor in science and mathematics in the Arlington High School, Illinois and assistant in zoology in the American E. F. University.

Richard A. Muttkowski, Ph.D. (University of Wisconsin), corporal signal corps, Instructor in zoology in the Kansas State Agricultural College and instructor in elementary zoology and comparative anatomy in the American E. F. University.

Don C. Simkins, B.S. (Denison University), private first class, instructor in science in Middletown, Ohio, and assistant in zoology in the American E. F. University.

These men had largely been ordered to Beaune on the strength of the record of their previous occupation indicated on their qualification cards and made an interestingly well balanced group in regard to their zoological interests. Fortunately these men believed from the start in the practicability of the work in which they were engaged which aided greatly in speeding up the progress of development. It was interesting to note the change

in attitude that many of the early pessimists underwent as the apparent impossibilities commenced to turn into realities.

Few departments had any conception as to the desirable courses to offer and no idea as to the number of students to plan for. In zoology the courses finally agreed upon were, elementary zoology, economic zoology (this course was never given), microscopical technique, advanced zoology (in which it was planned to arrange work to meet the needs of advanced students), genetics, agricultural entomology, histology and embryology. All work, except in the last two subjects, was planned on the five hour per week basis the laboratory periods being of two hours duration. It was desired as far as equipment and supplies were available to have the courses compare favorably with similar ones in the best American institutions and in general, I believe, this hope was realized. Histology and embryology were probably quite unsatisfactory to their instructor owing to the frequent shifting of the students by the college of medicine from which the courses drew most of their members. Had the life of the university lasted for another term of three or four months it is safe to say that all courses could have been exceedingly satisfactorily presented.

The department of zoology was fortunate in being able to find much necessary equipment on the grounds in the supply depot of the hospital center. This sufficed until the larger order from the main supply depot arrived about the middle of the term. It is not an exaggeration to claim that when the latter was unpacked the department was equipped as are few zoological laboratories in American universities. The photographs of stock room and laboratories give some evidence of this. All supplies were drawn from supply depots of the United States Medical Corps and were not specially purchased for the university. Among other instruments we had large numbers of scalpels and scissors of various sizes (sufficient to equip over two hundred students at one time), about one hundred and twenty microscopes fitted with oil immersion lenses and mechanical stages (machines worth about one hundred and forty dollars apiece to-day), seventeen new microtomes, two fine balances and so on through a long list of articles.

To expedite development members of the staff manufactured many things needed ranging from blackboards to erasers, platforms and reading stands. Heavy oak tables were supplied by the quartermaster and made excellent laboratory benches. Thousands of substantial folding chairs were available. Little trouble was experienced in collecting dissecting material.

FIG. 1. THE ABOVE PICTURE ILLUSTRATES THE TYPE OF ONE-STORY CONCRETE BUILDING WHICH HOUSED THE VARIOUS DEPARTMENTS OF THE AMERICAN E. F. UNIVERSITY AT BEAUNE, COTE D'OR, FRANCE. In the above building the Departments of Botany, Psychology and Zoology were located. These structures were approximately forty feet wide by one hundred and sixty feet long.

FIG. 2. A CLOSE VIEW OF THE FRONT OF THE ZOOLOGICAL LABORATORY WITH AQUARIA AND CAGES CONTAINING THE FAUNA OF THE REGION.

FIG. 3. THE INTERIOR OF THE BUILDING SHOWN IN FIG. 1 BEFORE SUBDIVISION INTO ROOMS.

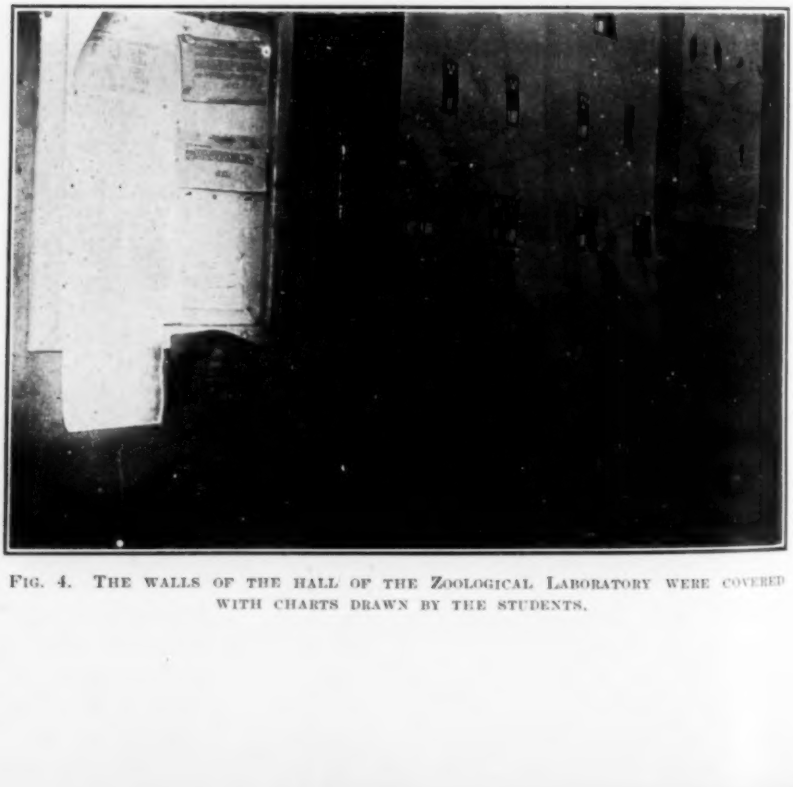


FIG. 4. THE WALLS OF THE HALL OF THE ZOOLOGICAL LABORATORY WERE COVERED WITH CHARTS DRAWN BY THE STUDENTS.

Within the confines of the camp was a large shallow pond in which great numbers of toads were spawning. It was a matter of but a couple of hours work at night to collect several hundred. Frogs were few and we did not catch more than a half dozen during our stay. The early spring with its heavy rains brought an abundant supply of big earthworms to the surface of the earth. A little later moles and mole crickets appeared in considerable numbers and were readily caught. Carp and pigeons could be bought in the game store in Beaune and there are probably few places in the world where a supply of cats and dogs are not available. Several hedgehogs were captured in

FIG. 5. A PRIVATE OFFICE.

April and May. The fauna was so interesting that shelves were put in front of the laboratory building and cages and aquaria were set up to hold the various animals brought in. Descriptions in French and English of the habits of the various forms were attached to the exhibits and this little zoo had a constant stream of visitors from early morning until late at night when I have seen would-be spectators striking matches to see what the "exhibits" were doing or to determine whether the toad and the snake still lived separately. It might be added to the credit of the snake that he at last got into the spirit of the affair and lived up to expectations. For several days thereafter the snake showed a considerable swelling in the middle of his

body suggesting the biological analogy that if base metals can not be transmuted into gold at least amphibians can be converted into reptiles.

A considerable number of excellent charts were made by certain students who preferred to put in the hour a day required in some service for the university in this manner. Since text-books in sufficient number for class use never arrived the charts were excellent teaching aids. The lack of class texts was not a serious handicap and the library was able to provide us with a number of well-known and valuable reference works which the department ordered earlier in the term.

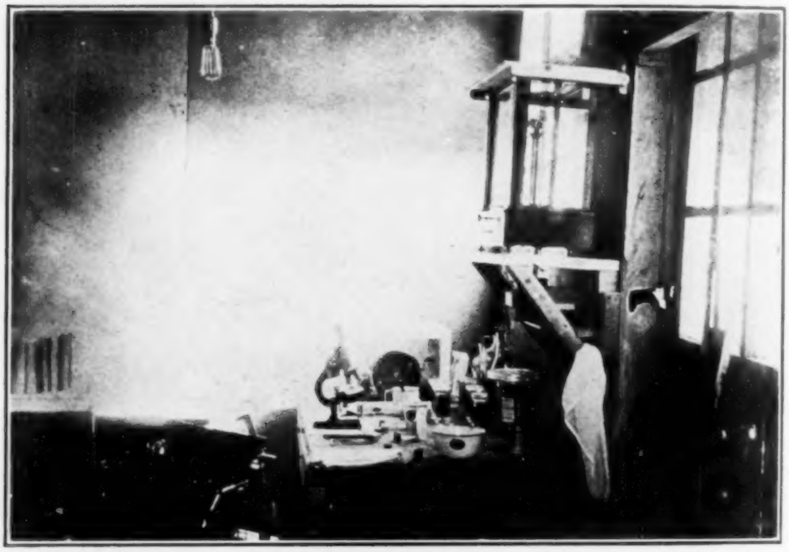


FIG. 6. A PRIVATE LABORATORY.

The largest number of students handled in the various courses was about two hundred and twenty-five, though toward the end the number was reduced to about one half through the departure of the men to join their Divisions which were returning to the States. It was the consensus of the instructors' opinions that though many of the students may have come to loaf they remained to work and that they had seldom had more interested audiences. Special lectures on the broader aspects of zoology were given in the evenings by members of the staff and attendance was voluntary and good. Though the interest and apparent caliber of the students were really high the results of the final examinations were disappointing. This it was believed might be due to any one or any combination of following

FIG. 7. A CORNER OF THE STOCK ROOM AFFORDING SOME CONCEPTION OF THE QUANTITY OF SUPPLIES ON HAND.

FIG 8. THE TOOL ROOM.

factors: the men were too recently from the unsettling conditions of war and army life to be able to take up serious and systematic study; that the little time or opportunity for evening work was possible because of the barrack living conditions and in some cases to more or less unnecessary evening inspections by certain group commanders and, lastly, the weather toward the close of the term was so delightful and such a welcome contrast to what it had been for about six months that every one wanted to be out of doors as long as possible.

FIG. 9. THE LABORATORY OF ELEMENTARY ZOOLOGY.

A very broad policy was formulated by the university of permitting forty-eight hour week-end passes of which all could avail themselves for trips to places of interest. To nearby towns truck or automobile transportation was available for special trips. The class in elementary zoology made one such trip in trucks of about eighty or ninety kilometers to Dole, the birthplace of Pasteur. The classes in botany were able to journey into the French Alps on a collecting expedition.

It is not to be inferred though that there were not some whose work was as consistent and as good as it would have been in America. At least one individual found the subject (zoology) sufficiently fascinating to signify his intention of entering the field permanently when he returned to the United States. Others worked outside of class hours in order that they might

FIG 10. THE LABORATORY OF HISTOLOGY AND EMBRYOLOGY.

FIG. 11. THE LABORATORY OF COMPARATIVE ANATOMY AND MICROSCOPICAL TECHNIQUE. The crates to the left of the picture contain unpacked equipment, chiefly microscopes.

FIG. 12. THE FACULTY OF THE COLLEGE OF SCIENCE.

get some experience in the use of instruments and methods that they thought were going to be of value to them in the work they intended carrying on at home. If these few were really reached it is fair to conclude that the brief operation of an American University in France demonstrated its practicability since it is but a small group that we are able actually to influence in normal teaching.

FIG. 13. TRUCKS CONTAINING ZOOLOGY STUDENTS ON THE WAY TO DOLE, THE BIRTHPLACE OF PASTEUR.

That the experiment was a success I think it safe to assert. That the students at large gained in a knowledge of the general if not always of the detailed content of the various fields of study was obvious and such being the case they could the more readily determine their reactions to these subjects and whether a pursuance of them in America would be congenial. More than this could scarcely be accomplished in the brief time available and if this was consummated the attempt at higher education in connection with the Army in France was, as claimed above, successful.

To the instructors, both commissioned and non-commissioned men, the getting back into educational surroundings and into what developed into a very fair academic atmosphere with a minimum of the military was a very delightful and a not soon to be forgotten experience. The almost complete absence of

that type of small-town, hopelessly provincial person who did so much to hurt the American reputation for fair play by refusing to admit any virtue to any foreign custom, people or scene was most cheering. Many of us had been commencing to wonder whether the vaunted broadmindedness was not a brain child of imaginative Americans. Fortunately the congregation of several hundred representative educated men completely dispelled this idea.

For the reasons indicated the writer considers the attempt to operate an American Army University in France as generally successful and a great credit not only to those leaders who built the institution but also to the American spirit of accomplishing what it starts out to do. This view recognizes the certain inherent difficulties of uniting educational work with military life and considers the difficulties to have been counteracted to a considerable degree by the uniqueness of the situation. Whether in a national scheme of militarized education the advantages will continue to outbalance the disadvantages is largely a matter of personal opinion and a subject too lengthy to permit of discussion here. Certainly though, we need a national and a standardized or uniform system of education.

THE PROGRESS OF SCIENCE

THE BRITISH ASSOCIATION AT CARDIFF

THE British Association for the Advancement of Science held its first meeting in York in 1831. The meetings were adjourned for two years during the war and the annual meeting at Cardiff was consequently the eighty-eighth. During this long period the association has adequately forwarded its objects which are thus defined: "To give a stronger impulse and a more systematic direction to scientific inquiry, to promote the intercourse of those who cultivate Science in different parts of the British Empire, with one another and with foreign philosophers, to obtain a more general attention to the objects of Science, and a removal of any disadvantages of a public kind which impede its progress."

The program at Cardiff maintained the high standards of the association, more especially in the addresses of the presiding officers and in the general discussions. The value of these addresses is shown by the extracts which the *Monthly* is able to print in its present issue, which give perhaps the best available statement of the progress and problems in the different sciences. The excellent organization of the work of the association is witnessed by the fact that through the courtesy of the speakers and the officers of the association we were able to obtain copies of the addresses in advance of their delivery.

There were 1,378 members present at Cardiff which is less than was usual before the war. Those in attendance consist largely of local associates who join for the meeting. Their fees provide a sum in the neighborhood of a thousand pounds which is annually appropriated to the committees of the association for the promotion of research, and they also supply audiences for the addresses and the meetings of more general interest and take part in the entertainment of the visiting scientific men. Owing doubtless to the different social situation the American Association has never been equally successful in enlisting the support of the people in the city in which it meets and it seems that changing conditions make it more and more difficult for the British Association to do so. Thus a correspondent writes from Cardiff that it is "rather disappointing to find the membership no greater. What is more disappointing still is that the principal reason for this is the apathy of local people of the educated classes to the presence of the association. The plain fact remains that it is the exception to find any one who has even heard of the Association."

The influence of the British Association in bringing scientific work to the attention of the country through the press seems also to be less than formerly and to be approaching the conditions in this country. In past years the London *Times* used to devote one to three of its large pages to reporting the daily programs whereas the space allotted has now shrunk to part of a page and there is a tendency to report the papers on social and educational subjects rather than the results of research in the natural and exact sciences.

The meetings of the British Asso-

PROFESSOR WILLIAM ABBOTT HERDMAN

President of the British Association and Professor of Oceanography in the University of Liverpool. The drawing of the Portrait has been kindly supplied by the Editor of the *Evening Transcript*.

ciation correspond more nearly to those of the American Association prior to the past twenty years. Our association then held its meetings in the summer, and excursions and entertainments were emphasized, which led to a larger attendance of amateurs and perhaps to more local interest. The American Association has now become primarily an association of societies rather than of individuals. No other country holds meetings at which so many scientific men are in attendance or at which the special programs of scientific papers are so extensive. It may, however, be that the more technical organization of the meetings has led to giving less attention to the work of bringing scientific research and its importance for the nation to the attention of a wider public. In a democracy science must depend on a wide appeal for its support and for recruits. The situation in England indicates the increasing difficulties as science becomes more highly specialized and scientific men become more completely absorbed in their special work. It should, however, be possible to apply scientific methods not only to scientific research, but also "to obtain a more general attention to the objects of science."

Professor W. A. Herdman is succeeded in the presidency of the association by Sir Edward Thorpe, emeritus professor of chemistry in the Imperial College of Science, London. The meeting next year will be at Edinburgh.

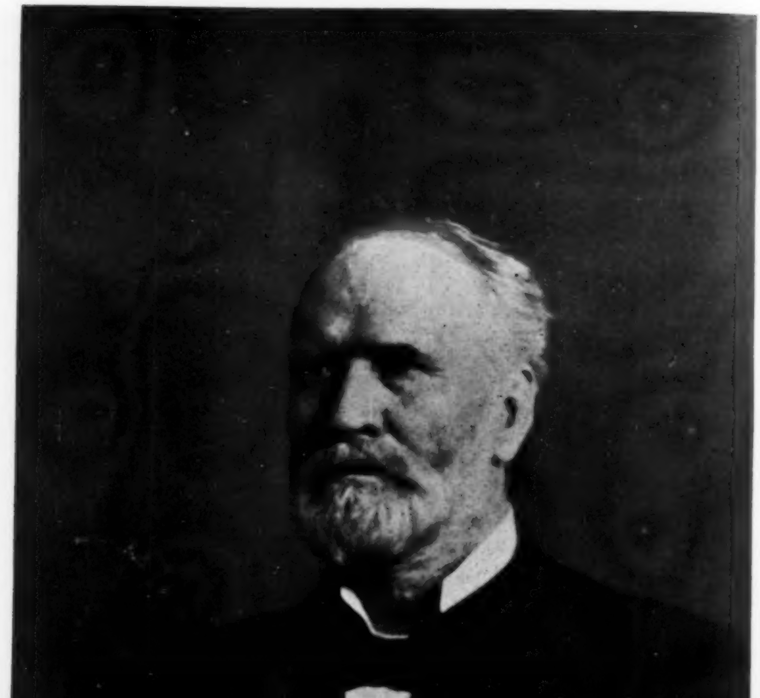
THE SCIENTIFIC INVESTIGATION OF THE OCEAN

As an example of the discussions at the meetings of the British Association that before the section of zoology on the need for the scientific investigation of the ocean, as reported in *Nature*, may be taken. Dr. W. A. Herdman, the president of the meeting, is professor of oceanography at Liverpool and his address on this subject naturally led to a fuller discussion with a practical object in view.

In opening the discussion, Professor Herdman pointed out the need of investigation under two heads—the scientific need and the industrial. He proposed that there should be a great national oceanographical expedition—that is, another *Challenger* expedition, fitted out by the British Admiralty, and embracing all departments of the science of the sea investigated by modern methods under the best expert advice and control. Such an expedition would require long and careful preparation, so even though the present time may seem to some inopportune to press such an undertaking, if this suggestion is received with favor by oceanographers, it might be wise to form a preliminary committee to collect information and prepare a scheme.

In the further discussion Professor J. Stanley Gardiner, Dr. E. J. Allen, Mr. C. Tate Regan and others took part, including Professor C. A. Kofoid from the United States and Professor J. E. Duerden from the Union of South Africa. Mr. F. E. Smith, director of scientific research at the admiralty, stated that his department had considered the question of a new *Challenger* expedition, and was of opinion that such an expedition was required, and he felt sure that the admiralty would take its share in the organization.

At the close of the discussion a resolution was unanimously agreed to pointing out the importance of urging the initiation of a national expedition for the exploration of the ocean, and requesting that the council of the British Association should take the necessary steps to impress this need upon the government and the nation. On the following day, at the committee of recommendations, this resolution also received vigorous



JAMES WILSON

Secretary of Agriculture in the cabinets of Presidents McKinley, Roosevelt and Taft, previously professor of agriculture in the Iowa State College and director of the Experiment Station, who died on August 26, at the age of eighty-five years.

WILHELM WUNDT

Professor of philosophy in the University of Leipzig, leader in the foundation of psychology as a science, who died on August 31, in his eighty-ninth year.

support from other sections, *e. g.*, those dealing with chemistry, physics, geology, and geography, in all of which, as well as in zoology, investigations are required which could be undertaken by such an expedition. The general committee of the association recommended the council to appoint an expert committee to prepare a program of work and to consider the personnel and apparatus required.

At its last two meetings the Pacific Division of the American Association has given special attention to deep-sea investigations and further emphasis was placed on the subject by the Pan-Pacific Conference held last month at Honolulu. It would be desirable in the present international situation for the United States to cooperate with Great Britain and its Dominions in a thorough scientific exploration of the seas.

SCIENTIFIC ITEMS

WE record with regret the deaths of Joseph Paxon Iddings, formerly geologist of the United States Geological Survey and professor of petrology in the University of Chicago; Samuel Mills Tracy, agronomist of the United States Department of Agriculture; Dr. Walter Faxon, until recently in charge of mollusca and crustacea in the Museum of Comparative Zoology of Harvard University; Ellis L. Michael, zoologist of the Scripps Institution for Biological Research of the University of California; Benjamin Smith Lyman, geologist and mining engineer of Philadelphia; John Percy, professor of mathematics at the Royal College of Science, London; Sir Norman Lockyer, director of the Solar Physics Observatory, London, and editor of *Nature* from its establishment over fifty years ago, and Wilhelm Wundt, professor of philosophy at the University of Leipzig, where he established the first laboratory of psychology.

DR. GEORGE ELLERY HALE, director of the Mount Wilson Observatory, has been elected one of the twelve foreign members of the Società Italiana delle Scienze, in succession to the late Lord Rayleigh.—Professor Raymond Pearl, of the Johns Hopkins University, has been decorated by the King of Italy as Knight of the Crown of Italy.—Professor R. Roux, director of the Pasteur Institute at Paris, has been awarded by the United States government the Distinguished Service Medal for especially meritorious and distinguished service which was of great consequence to the American Expeditionary Forces.

COLONEL F. F. RUSSELL has resigned from the Medical Corps, U. S. Army, to take charge of the newly organized Division of Public Health Laboratories of the International Health Board of the Rockefeller Foundation. — Dr. Charles Hubbard Judd, head of the department of education of the University of Chicago and director of the school of education, has been made chairman of the department of psychology to succeed Professor James R. Angell, who resigned to accept the presidency of the Carnegie Corporation of New York.

YALE UNIVERSITY has received from an unnamed graduate a gift of $3,000,000 to the general endowment of the university, contingent upon additional gifts of $2,000,000 by next January, exclusive of those through the alumni university fund. The gift is made to meet increased faculty salaries.—Cornell University has received a gift of $500,000 from Mr. August Heckscher, of New York City, for the endowment of research. The income of the fund created by Mr. Heckscher's gift will be used to maintain research professorships and to provide facilities for scientific work.